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The Psychological Review

THE SELF AND MENTAL PHENOMENA

BY ROBERT MacDOUGALL

New York University, New York City, N. Y.

The study of any subject matter may be approached from a variety of standpoints. Each inquiry is guided by a specific purpose which determines the conception of its data as well as the nature of the conclusions to which it leads. This multiplicity of viewpoints is the source of errors which must be guarded against in discussions of the methodological postulates and limits of any science. In the history of psychology a notorious instance of the failure to observe these logical distinctions is to be found in the confusion of metaphysics and descriptive science which underlies the procedure of rational psychology. In contemporary discussion the conception of data and criteria is exposed to error from a similar interpenetration of principles.

The mental life may be considered from three points of view, the practical, the scientific and the philosophical. The first inquires concerning the immediate uses and direction of the mind, the second concerning its structure and development, the third concerning its metaphysical significance. The practical student views mind as an instrument, the scientific as a phenomenon, the philosophical as a real. The first is interested in its external relations, to things and other individual minds conceived as ends of the practical intelligence; the second is interested in its internal relations, conceiving it as an organic system having a typical form of activity; the third is interested in its relation with ultimate reality and inquiries concerning its place in the universe as a whole.

For practical purposes the individual mind is plastic material to be moulded in adaptation to an end; for the scientific it is a class of phenomena to be understood in terms of reciprocal relationship; for the philosopher it is a system of rational activities and ideal purposes to be interpreted in terms of their absolute values. The points of view and aims, the methods of treatment and resultant products, are unique in each of these several ways of regarding the mental life.

In one sense the practical man is not interested in the study of mind at all. His object lies wholly beyond its circle in some change which is to be brought about in the external world. He wishes to sell his merchandise, to obtain good service, to modify social judgment, to secure political victory, or the like. In the attainment of some practical ends other human minds are not involved in any direct way. One deals only with physical materials, as in raising vegetables or making a chair. But in most of the things at which one aims other human wills must be considered. The end is to be attained only through their mediation. One needs their assistance or must overcome their objections,—that is, they appear as necessary instruments or as accidental obstacles to the accomplishment of a purpose. They are to be informed, taught, trained, encouraged, assisted; or they must be persuaded, convinced, circumvented or defeated.

In all their more significant forms our ideals are thus incarnated through the mediation of other human wills. The character of these wills must therefore be considered in our plans. To misunderstand them involves failure because our object can be attained only by allying, transforming or overcoming them. Though we may have no interest in these minds as such, either as systems of phenomena or as purposeful and idealizing selves, yet we must take them into account in our reactions; and thus to take them into account involves understanding them. We must apprehend both the present point of view of the mind with which we deal and its permanent habits of thought and feeling, if it is to be successfully directed or utilized in the furtherance of our aims. A knowledge of men, we say, is essential to success in the world f affairs.

In acquiring this knowledge attention, nevertheless, is not turned to the mind involved but to the end to be attained, as the fencer gains skill not by regarding sensations and movements in the arm which is lunging or making a parry, but through fixing his eye upon that of his adversary and countering or responding to each motion he makes. The mind is not first studied in isolation and the principles thus acquired applied in subsequent dealings with it. Knowledge springs from actual conflict with other minds in the pursuit of the ordinary business of life. While it thus becomes necessary to study these minds, the examination must be carried on as part of the conflict itself. It is a study determined, as to its form, by the requirements of the moment, and as to its degree, by the significance of the practical motive which prompts it. Such knowledge is never an isolated system of truth but in its very origin and interpretation is unified with a system of ends. Never abstract, seldom formulated and then only in a proverb, practical knowledge represents from the outset an essential unity of understanding and application, of knowledge and purpose.

On the other hand, in so far as each moment is dominated by a specific practical demand, there will be studied only that feature of the mind which has significance for this ulterior purpose; and as progress is made toward its realization the characteristics of mind which come into review may shift in a wholly illogical manner. Thus no opportunity is afforded to develop any characteristic systematically in its relation to the rest; no leisure, in the pursuit of practical purposes, to undertake a rational study of the mind as a whole or of any individual aspect or function of it. Practical experience gives a series of vivid impressions of the minds we encounter but no systematic knowledge of their structure. It requires alertness, shrewdness, a capacity for swift intuition of the mental attitude and its bearing upon the matter in hand; and it tends to the cultivation of a mind full of useful maxims, vigilant, retentive of impressions and instinctively wise in the conduct of affairs. For adaptation turns not upon a consistent view of human nature but upon specific informa-

tion and the ability to respond with a definite reaction to each situation as it arises.

The knowledge gained through active participation in experience as a system of ideals and materials for their realization—the education of life, as we call it—has both excellences and defects characteristics of its origin. It is a knowledge won through activity and determined by utility, but it is essentially fragmentary and unsystematic. Practical knowledge of the mind is not the outcome of a consistent attempt to understand the phenomena with which it deals as a whole. In the last analysis, therefore, it provides an insufficient basis for even that practical reaction which it served originally to make possible. Its content is made up of those individual items of information which have been found useful in the attainment of practical aims.

There is a conception of the mind, likewise to be included within this first category, in which the treatment is not instrumental to an extrinsic aim but regards the mind as an end in itself. It is that form of study which is involved in educative discipline and direction. Here, then, is an instance in which it would seem that interest centers in the mind as such, and that the latter may be called the immediate object of study. In this case the essential assumption is indeed the significance of the mind's development as the subject of experience. The training of the mind may be called a means to an end, but its discipline and culture are not of this nature. They rest upon the conception of personality as a subject of worth in itself, though they may involve other things as well, such as the functional contribution of the individual to a system of social ends. Educational interest may thus be said to center in the nature of the mind itself and to seek nothing beyond that mind in its study.

Nevertheless the locus of interest in education lies not in understanding the mind but in developing it, in bringing it to its full realization. The determining conception is to be found in that ideal system of functions and attitudes which the fostering intelligence seeks to develop. The mind as it comes under review by the educator, whether parent, teacher

or priest, is thus conceived in relation to an ulterior reality; and its study is, after all, instrumental to the purpose of attaining a practical end. In neither utilitarian nor educational attitudes is the system of mental processes as such the object—the final object—of study.

In the scientific treatment of mind it is just that complex of phenomena as presented to observation which is under consideration. In its view the individual mind is neither something to be reacted to and made use of in the give and take of vital experience, nor is it a reality which must have place and significance in the sum of things. The scientist is concerned neither with utilitarian modifications of the mind, nor with its ideal valuation, nor with the metaphysical interpretation of its existence. As subject matter for science it is a system of materials and processes definable in terms of the qualities and forms which they manifest but belonging within the general field of describable relations which phenomena at large present. The aim of the psychologist may then be defined as the explication of the content and form of consciousness as a process in time.

This, however, is not sufficient. The system of regulative conceptions under which mind is to be viewed must take its character from the technical purposes of the scientist and the consequent way in which the object of his study is to be defined. The field of descriptive knowledge is in general that system of intelligibly related fact which we call the world of natural law. On the existence of orderly processes the very possibility of knowledge is founded, for only such a world can be conceived in terms of logical concepts. Anomy, chance, caprice are terms by which we mark the absence of those conditions which make science possible. This representation of the world as a system of orderly phenomena is of course the condition of all rational conduct as well as the prerequisite of intelligibility in the object of knowledge.

Of all the fields to be explored by the scientist there is none in which law holds a more significant place than conscious life. It is the general subjection of its materials to ideal order which gives to human existence its characteristic

form and value. Mental life is a unity of functions. Whether a single attitude be considered or the synthesis of successive experiences, its fundamental quality is to be found in the system of significant activities which it constitutes. The consciousness of any moment, for example, manifests a unity in the midst of its obvious complexity of make-up, which can best be exhibited by contrasting it with the essential discontinuity of two individual minds, however near their approximation in content and environment. Each moment of experience, in the second place, stands in intimate relation with that which precedes and that which follows it. As the content of any moment forms a complex within which no single element can be modified without affecting the character of the experience as a whole and of every element within it, so does the content of each moment enter into a series of mutually modifying complexes. Memory holds this series together in its retrospection and gives an added significance to each event by its reference to the system thus imaginatively reproduced. This series of associated experiences, in the next place, forms a system having connection at each moment with the objective world with which it enters into significant relations both by way of adjustment to its changes and by modification of its course. The adaptation of means to ends which the mental life presents, finally, is the expression of a system of rational purposes and ideals which give to experience that consistency and value in which its significance rests.

Human experience is thus not simply a succession of phenomena, of events in time; it is a unity of functions manifested in an orderly and rational process. The mental life is an expression of ideals of law and order. Continuity and rationality appear there as in the system of external phenomena. The relation of order and law to the content which is organized under these forms of the mind, is, however, unlike in the two cases. Unity and rationality are attributed to the external world; they are experienced in the inner order. The unity of the outer world is either a logical concept or a metaphysical postulate; that of the self is a reality of immediate experience.

The phenomena of the external world appear to us as causally determined events. Whatever aspect of significance they may have necessarily escapes the observer. If there be a world-subject—as the self is a subject—which gives to these phenomena a unity and value, it is at least not a reality for our experience. Such unity as we predicate of the phenomenal world must arise from the synthetic activity of our own minds in construing these events, and it is simply attributed to that world as the logical condition of its existence as a system of phenomena. In other words, it is solely a methodological postulate. The unity which we predicate of the mental life, on the other hand, is not thus logically conceived and referred to a system of elements presented as a mere succession of events in time. Unity is an immediate and indefeasible reality of our experience. It is a fact,—if that of which one is thus immediately aware can be called a fact—whose existence is neither posited nor inferred, but intuited. We may thus say, without reservation: There *is* meaning and rationality, law and unity in the mental life, because we find them there.

The mental life therefore seems to present, in its essential form and value, just those characteristics which must be possessed in order that any subject matter may be susceptible to treatment under the general conceptions of science. Yet it is just in this regard that the scientist finds his way closed; for the limitation imposed by his point of view appears within the fields of physical and mental science alike. The methodological unity in terms of which the external order is construed can never become an object of descriptive science. It is not a fact among facts, a phenomenon which can be isolated and regarded apart from the system of things to which it gives unity; nor is it a relation among things, whether of coëxistence or succession, of cause and effect, of dependence or support; nor is it a law expressing such relations in their most general form. As science deals only with facts and their relations, which it expresses in terms of concepts and laws, the metaphysical unity which is apprehended in connection with our intuition of the external world can never become an object of

scientific treatment. The unitary and significant world which metaphysics postulates necessarily becomes a series of phenomena; this can be formulated only in terms of abstract units of constitution which in turn can give nothing but a system of conceptual laws as its result.

The unity of the mental life, similarly, lies beyond the reach of scientific treatment. It is a reality for which the terms self, I, person and so on, are expressions,—the origin, for each of us of all such rationality as we find reflected in the world of experience. It is this unity of the self which makes every part of life significant and gives it value as a whole. It is in its service that the world of experience arises, that its phenomena are studied and their laws formulated, that its materials are transformed and organized under ideal criteria, that the world is conceived as a rational whole. In this sense of unity and value in experience the self is universally expressed. It functions in every conscious perception and act. To the one it gives rational significance, to the other purpose and meaning. It defines and directs, inspires and energizes, subordinates and organizes. It is the unity in which alone each element of the mental process finds meaning. The self is related to the individual elements of the mental life, and to their sum, as the soul's relation to the body has been expressed: It is all in the whole and all in every part. No mental activity exists which does not manifest it; no analysis of experience will ever bring to light a mental event in which it is not postulated.

This relation must be recognized at the outset. Self and event are elements in a single reality which we call an experience. In abstraction from its correlative neither of these has existence; the experience must exist for a self, the self must be realized in a succession of experiences. It is under the conception of self, in this meaning of the term, that all mental facts are treated. Of floating psychical phenomena, that is, of facts not given to a subject—which we describe by the single word 'experience'—we know nothing. In psychology therefore, as in practical life, the conception of self is the ultimate reference in every individual constituent;

for it expresses the final unity of the system of phenomena with which reflection deals, as in immediate intuition it constitutes the unity of experience in the real subject. But in this sense it is not a phenomenon which can be treated in terms of its relation to others in a common universe of discourse. The self is the *summum genus* of the psychologist, the theoretical concept which expresses the necessity he finds for a common reference in all the phenomena he considers. Every mental fact is the experience of some self, it is part of the context of a mental life. But within this field each experience of the self, as conceived by the psychologist, must become an event in an historically conditional series.

It is in this sense that the term is used when we speak of self-existence as something that can essentially be given in a single experience. I am aware of my own existence, we say; I am conscious of myself; I know myself as a subject over against that with which, as subject, I deal in any moment's experience. These phrases are common and carry an important meaning. For the psychologist, however, it is not their practical or metaphysical value which is to be considered, but solely the nature of the fact which is indicated by them. If the awareness of one's own existence be a phase or element of experience, it should be possible to point to the occasion when it arises and to say what kind of an experience it is in which the self is thus immediately revealed. To this in such a case it is answered that every individual experience reveals it. I can be aware of nothing without being aware, at the same time, of myself as knower; I can suffer no pain and enjoy no pleasure without being conscious of myself as subject: I can desire nothing, seek nothing, regret nothing, without postulating myself as the subject of these attitudes of will. The sense of self accompanies and grounds all experience.

But thus conceived self-consciousness is no longer a fact among facts. It is not an experience which can be differentiated from others and studied psychologically. It is equally given in all experience, yet is not a constituent of experience as is sensation, for instance, or affection. It

cannot be made more clear by dwelling upon it, for it does not rest upon a specific content which may be seized and held before the mind. In short it can only be ascribed as the immediate and irreducible reality which is itself self-existence. If there be anything that is metaphysically simple it is this, the universal form of subjective reality. When used in this way, therefore, the term 'self' means nothing more than is implied in the phrase 'conscious fact'; it is simply the postulation of the self as the formal condition of all mental phenomena.

If the term self be used to stand for the system of related characteristics and activities which psychology studies, it has a meaning and value for that science; if it stand for a specific form of experience which can be pointed out in any individual mental life, it may be approached and studied by psychological methods; but as the universal and necessary ground of reference in all individual experience it does not properly fall within the psychologist's field at all. The barren reassertion, in connection with each fact discussed, that it is the experience of a self adds nothing to its treatment; and the indication of this relation should appear in that preliminary definition of the field of psychology itself which must always be carried in mind but needs no subsequent repetition.

There is one class of cases, however, in which these two points of view meet and the subjective unity of experience is projected into the outer world of discrete phenomena, not as a mere logical postulate for reflection but as an interpretative basis for the practical reactions of the will. The mind of another person, however it may be known or presented to him, is not immediately experienced by me. Neither as a systematic unity nor as a momentary self-feeling is it thus given. What is known, and what alone I can observe, is that variety of physical facts which I call expression, speech, gesture, posture, movement, and so on. They compose a succession of bodily attitudes and reactions, now of one complexion, then of another, now occurring in one relation, then in a different one. My observation of the facts in question —all namely which I have in mind which I speak of the ex-

pression of character—is disconnected, partly because of the intermittency of attention, partly because of the obscurity of many factors of the series. As impressions they contain scarcely a hint of that unity which they possess for my mind when I conceive them as the characteristic expression of a self. They are neither contiguous nor correlated; they occur as discrete happenings; and what is involved is, qualitatively, part of the world of sensible objects. They are not even the *disjecta membra* of a self, such as the events of one's own life would be were one to conceive knowledge of self to be the product of logical reflection upon experience, and not an intuition given in that experience itself. Before we can approach the problem of a self objectively given we must construe each complex physical change in terms of a mental correlate to which we attribute it; we must, in other words, conceive it as the expression of a particular mental attitude. Thus construed we ignore the character of the reaction and deal thenceforth with the subjective attitudes implicated, which we then connect by means of the principle of identity, conceiving the various acts as members of a common system of expressions, the manifestation of a particular self.

The conception of a self, in all such cases, is an interpretative attribution,—though not a reflective formula—by which meaning and unity are given to a diverse and disconnected series of acts. We do not, of course, rest with the postulation of a bare principle of unity, but proceed to reconstruct from these fragmentary data the specific characteristics of the self in question, and even to predict the form of expression to be expected in the future. In all this procedure, though the materials are presented discretely as scattered events and must receive formal unification, the conception of a self in such cases possesses more than a merely regulative value. It is not, as in science, an abstract synthetic formula according to which we unify a specific group of facts. The result, therefore, is not a logical formulation but the predication of a real being. What we do is not to treat events as if they were the expression of a self, but as the manifestation of a self really existing. Logically, in all our dealings with

other human beings the other pole of our relation is such a self or real subject, identical in general character with our own being; though this attitude is never systematically maintained but alternates with a treatment of them as objective or instrumental material.

In the case of one's own experience, no less than in the interpretation of behavior in other men, the concept of self needs definition. The term may denote a unifying principle or a unitary being; it may indicate either the product of logical reflection or the significance of immediate intuition. Concerning its value for descriptive science no question can well be raised. It has been used to denote a conception as indispensable to psychology as the concrete and dramatic representation of individual character is indispensable to our adjustment where the practical reactions of other human beings are concerned. But in the case of each of these two specific contents which the term self may receive we must consider what status and value it possesses in relation to the methodical aims of psychology.

Every event in the mental life may thus be conceived from either of two standpoints,—in terms of its content and relations, and in terms of its form and significance. In the former case it is treated as a phenomenon to be described; in the latter as an experience to be interpreted and appraised. The description of phenomena, as has already been pointed out, is made possible only through their resolution in terms of a constituent unit. The interpretation of experience is necessarily based upon the presupposition of a purposive and rational will in terms of whose ideals it is unified and receives value.

These standpoints cannot be combined in a single intuition. The event must be treated in terms either of its existence or of its worth; no middle course is possible. The two points of view are mutually exclusive, for they represent different purposes and result in products which cannot be compared or brought within the same system. The one point of view is exemplified in theoretical interest, the other in practical and moral activity. The former conceives the

mental event as a problem for the understanding, the latter regards it as an object of the moral or æsthetic will. To conceive the event as a phenomenon is, in general, the point of view of natural science; to regard it as a significant experience in the life of the self is that of the real subject.

Personal experience is intelligible only when conceived in terms of a significant process in which, through reaction upon a conditioning and modifiable world, certain practical and theoretical ideals are realized. The primary aspect of all experience is this rearrangement of its materials in the service of an ideal order. The specific content of any such ideal must be stated in terms of the subject of experience and its demands, whether the organization be practical or sentimental or logical. The forms of organization comprised by the cycle of experience are thus never to be referred to objective determinants, such as the recurrences and juxtapositions which are to be found in their material elements. Every unity of experience reflects the synthesizing activity of the self which is universally originative. To refer it to the unities of the world of physical stimulations is unthinkable.

Our perceptions, in which the phenomena of sense-impression are organized into a system of things, are not to be explained by any analysis of the field of sensation. Their demarcations and combinations cannot be expressed in terms either of the momentary correlate which they possess in the world of sensations, or of the history of its constituents as elements in the past experience of the individual. Whether we consider the qualitative aspect of sensations and the logical resemblances and differences which they present, or the constellations in which they are grouped and the serial interruptions which mark their course, the result is the same; no adequate explanation of the forms of organization which appear in the perceptual world can be found in the connections and sequences which the field of sensations presents.

Memory, similarly, is not a reflection in consciousness of the order of past impressions. Neither its architectonic principles nor its function in individual experience can be referred to the uniformities of coexistence and succession in

the field of stimulation, or to the order of impressions which they occasion. Memory is an originating and selective activity which at each moment reorganizes the materials of past experience in the service of a present demand. It does not represent the content of an earlier moment nor reproduce the actual successions by which experience was originally marked. All memory is synthetic and productive, as well when we recall those phases of past experience which are to be brought into practical relation with the present as when we give new organization to the materials which imagination affords in the service of some logical or æsthetic ideal. The distinction between reproductive and creative types is a discrimination in teleological relations and not in the ultimate nature of the processes involved.

Nor is the system of connections which our thinking presents at any moment to be explained in terms of the so-called laws of association. The web of rational thought is no product of adhesion between contiguous ideas, no result of the frequency with which ideas have recurred together in past experience. As well try to explain the relation of hewn stones in a building by reference to the contiguity of rock-masses in the quarry, or the frequency with which pairs of blocks were laid side by side on the various vehicles which transported them. In proportion, one is tempted to say, as an individual mind is dominated by the historical succession in which its materials have been presented,—by what we call the routine of past experience,—the less will be its unity of theoretical organization and the less its practical efficiency in reaction,—the less, in a word, will it be able to think. These, and all other forms of organization which the mind exhibits, must be referred to a wholly different origin, namely, to the ideals of the self and the system of purposes which its life comprises.

If, therefore, we seek a key to the forms of organization which mark either the momentary or the habitual attitudes of the individual self, it must be sought not in the system of materials presented,—such as the immediate field of sensations and the physical or social milieu in which the subject

lives,—but in his systematic dealings with such materials; in a word it is to be discovered in the characteristic reactions through which the attitudes of the self are expressed. That is a unity, for example, which (however it be made up) I treat as a single thing; and that is dual which I treat in terms of two distinct reactions, whether its constituents be more manifold than the former or less so. As the most generalized form of the self's reaction upon the world is the singling out of a particular complex of elements to be the object of the moment's conscious activity, this might be expressed by saying that that is a unity which is the object of a single act of attention. If we are to seek a specific system of correlatives with which to relate the forms of organization which occur in our experience, it must be in the succession of acts in which we deal with the materials of intuition, not in the character and relations of these materials as originally given in sensation.

But this must not lead us to regard the system of reactions as original any more than the system of impressions. The formal unities which the mind presents are no more the mere reflection into consciousness of the reaction or bodily attitude which the stimulus occasions than they are the product of the physical constitution of the stimulus itself. The latter concept, like the former, eliminates the subject from the equation, and makes mental organization the result of changes in the external world. It is thus wholly unserviceable in the work of interpreting the nature of experience. It makes the self as purely a derivative phenomenon as when we attempt to describe the unities of experience in terms of the materials unified. The reactions in question embody forms of organization under which the self has conceived the materials thus presented; they do not give rise to them. The reactions are derivative, not original; instead of producing they are the result of those formal unities in terms of which the self apprehends or modifies the materials of experience.

It is thus evident both that we must approach the self through the system of reactions which constitutes its response to stimuli and that we must treat these reactions as flowing from, not determining, its character. The arrangement of

materials primarily offered must in the end absolutely fail us here. How the sculptor conceives a block of marble appears in the figure he carves from it, as do the conceptions of builder, painter, roadmaker and geologist in the respective modifications they produce. These characteristic reactions find their typical manifestation in human language, through which alone they can be satisfactorily made known; for all those other embodiments which the mind's unities receive through the organizations of materials are but inadequate approximations. I unify the multitude of plants which a summer field presents when, having studied their characteristics, I arrange them in groups according to principles of classification; but this reaction upon my materials cannot be completely embodied in the grouping of specimens in beds or herbaria. It finds adequate expression only in the botanical system of nomenclature which language makes possible.

The centrality of this conception in all our ideal affairs is beyond dispute. Individual life is the realization through plastic materials of an active self which expresses its nature in characteristic and unitary ways. But the distinction between such a concrete unity as confronts us in this field and that abstract unity which the psychologist seeks needs constantly to be redrawn, just because the former has primacy in experience and the psychologist himself slips only too easily into his habitual way of construing the materials of intuition.

It has been pointed out that any mental event may be treated in terms of either its content and relations or its form and significance; in other words, it may be conceived as a phenomenon to be described or as an experience to be interpreted. This may perhaps be illustrated by developing the latest oppositions in certain unifying concepts which are almost indifferently applied in descriptive psychology. In connection with the following statement it is to be remembered that in formulating such implications one must of necessity go beyond the uses actually made of the concepts in question and may seem to do violence to terminology.

The complexity of the field of consciousness appears under

a two-fold aspect. Quantitatively it is represented in a multiplicity of elements; qualitatively it appears in a diversity of constituents. The numerical complexity of the content of experience is resolved in the concept of association; its qualitative complexity is resolved in the concept of the self. The former is an abstract synthesis of materials, the latter is a concrete unification. The concept of association is the result of generalization from experience,—it is a descriptive formula. The self is a substantial existence, which may be defined as the real subject of experience. The term association thus serves the same purpose as every other abstract concept in psychology; it indicates and unifies a specific system of phenomena. The term self also unifies the content of experience. As each mental state exists only in connection with a modifying system of associates, so the self's experience at any moment is single.

The term quality, as used here, does not refer to those sensory differences which separate the various orders of sensation, visual, auditory, and the like; nor to those which appear within the limits of each several sense and form a series of variations such as hues or tones. These differences are called qualitative, but they serve only to condition numerical distinctions within consciousness. Discrimination turns upon specific sensory differences, call them variations in intensity, vividness, quality, extension, or what one will. Any two things must *be* unlike—whether they are objects in space or events in time—if they are to be distinguished. The specific nature of the difference is inconsequential. The immediately felt sensible unlikeness must be there if the field which is unified in association is to be conceived as numerically complex. This form of qualitative differentiation is but one aspect of the field of consciousness which is quantitatively manifold. Its resolution therefore raises no new problem of unification.

But there is another series of differences, not to be unified in this way, for which the conception of the self or real subject is invoked. The content of experience cannot be reduced to a representative system alone. It is not comprised

in sensation, image and concept with their connections. Thought, if the term be used to describe this aspect of the mental life, is but one of several sides which that life presents. Traditionally its scope has been expressed in terms of three concepts,—cognition, affection and volition; or thought, feeling and will. None of these is pure. Feeling is never blind or passive; thought is never passionless or without result; action is never motiveless or objectless. Each term represents a logical resolution of the moment of experience in terms of a single conception. No one of these elements is chronologically isolable. They do not represent states of mind but aspects of the inner life. The experience of any moment necessarily involves all three constituents.

The coexistence of these qualitatively different phases of mental life raises a new problem of unification. Descriptive formulations fail to meet the situation. The concept of association is inapplicable, for as thus construed these aspects of self-activity are not constituents of a single field of data, such as must be given in connection with the psychological object in all its transformations. They are not temporally dissociable, as are successive events; nor mutually exclusive as are objects in their spatial arrangement; nor subject to logical redistribution as is a series of tones or of concepts. In order to bring these three aspects of the mental life into relation one must turn to the conception of their significance in the unity of functioning which that life presents. In the experience of a self each of these phases has a characteristic place. Feeling gives value to experience; will modifies experience in the service of feeling; thought guides the will in its activity. Out of the domain of feeling arises the system of ideals which the mind possesses; will is the continuous attempt to realize these ideals; thought reveals the world as a system of means by which the ends set by feeling may be attained. That the wish is father to the thought is true in a fuller sense than the proverb intends; for the thought is, pragmatically, a stage of the realization of the desire. It is not simply a thought—a mental representation the significance of which is exhausted in its own inner content—it

is a reconstructive programme, a plan of action which has for its object the re-making of the world in some particular.

Thought is thus indissolubly joined with action on the one side and with feeling on the other. Not only does thinking pass over universally into doing, it is also the necessary condition of action. Happiness does not attend upon the soul like a shadow upon the movement of an object. It may be posited as the soul's ideal state, that in which only it rests; but dissatisfaction with an existing situation does not banish or induce the ideal condition. The soul may be miserable and know no way of escape; as it may be happy—that is, find the state in which it exists ideal or pleasurable—without having striven for its attainment. Happiness and unhappiness describe positive aspects of the concrete consciousness, irrespective of the relations in which it stands to the process of transformation. The discomfort of a toothache is a fact whether one know how to seek relief or not. Even when the representation of the ideal state is possible—as when one has had experience of a happy condition of which he retains remembrance when a state of misery supervenes—the envisagement of the ideal does not make it real. The end sought by the will, in such longing, is not realized by a simple fiat. The situation must be changed by some reaction upon the environment, not by the existence of the inner discontent or desire for another state. There is always something to be done. That quality of consciousness which is represented as ideal is attained only as the consequence of an act or series of acts terminating, objectively, in a changed world. To recognize an objective order and to exist in it means just this. That world is a system of reality which the mind does not create, which does not reflect faithfully the transformations in the individual consciousness—that is, it does not constitute a pure objectification of the self's activities, a world which exists simply in virtue of the appearance in consciousness of a structure of ideas.

At the same time the self finds the objective world plastic to its purposes. It is neither the shadow of subjective movements nor are the latter simply a reflection of its fixed order.

The world is a system of means which conditions the self's realization of its ends; and the modification of the objective system is followed by a transformation of the subjective. The attainment of the whole system of ideal ends which life comprises, whether practical or theoretical, utilitarian or æsthetic, is thus conditioned by the use of materials which the external world affords, and its procedure must be subject to the order which that world manifests. To discover the uses to which materials may be put and to formulate the laws of the natural order is the function of thought. The soul seeks perfection or happiness (whatever name may be applied to its ideal state) by all the means within its power. At the outset of its history these means are lacking; the soul is either unhappy without knowing why and without being able to help itself, or it is happy without referring its happiness to an object which it possesses or an objective condition which affects it. The dependence of its states upon such conditions and the connection between means and end it must learn empirically. This we call the development of experience. The soul must thus both achieve its own happiness and create the means by which this achievement becomes possible. This means is, in a word, knowledge. It is the product of that activity which we call thought.

The development of mental content is thus chiefly an elaboration of the process of thought. It is an increase in the understanding of the world and its relations as a system of means by which the ideals of worth may be realized, the progressive enrichment of the soul's apperception of the world as a synthesis of plastic elements. It is this fact which affords such justification as exists for the unequal distribution of topics in psychological discussion; for the largest constituent of all general works of psychology is cognition in its manifold aspects.

In the unification of feeling, thought and will which is thus achieved the resultant product is of a different order from that which appears in association and its transformations. The latter are all synthesizing concepts which the mind has devised in the course of its attempt to describe subjective

phenomena. Attention, association, memory and the like are not hypostatized and given an objective reference. Each is but a formula by means of which certain observations have been brought together or generalized. They represent the association-complex or historical connections in which the fact exists, and—for the psychologist—nothing more. In the case of the self, conceived as that unity of functioning which gives meaning to the whole system of psychic elements,—which not only assigns its place to feeling, thought and will respectively, but also constitutes the ground of reference in evaluating every specific activity of the mental life—no such descriptive generalization is intended. The self is not an abstraction but a concrete being. Its existence is not conceptual but substantial. It has an objective reference, not simply value for the synthesizing mind. Its essence, in a word, is intuitionally real, not phenomenal.

Conceived in this way the term 'self' does not stand for a system of subjective phenomena, nor are thought, feeling and will empirically related facts in such a system. They are construed solely in terms of value, either through a reassertion of the immediate preferences of vital reaction or on the basis of some specific metaphysical interpretation, as in ontological voluntarism or intellectualism. The constituents of the self, in this meaning of the term, have therefore no status as data for mental science, nor does their unity express those descriptive and genetic relations with which it is concerned. The point of view which psychology represents conforms to the system of general criteria which descriptive science involves. It deals with the world of subjective experience as a system of phenomena, to be described in terms of its elementary constituents or explained through the determination of its historical relations. The necessary abstractions of science reappear in the treatment of all subjective data. In psychology as in the physical and biological sciences only facts and their relations can be considered. The significance of the world, whether objective or subjective, is irreducible in terms of science.

If it be objected that when thus conceived mental states,

singly or in successions and systems, are a mere abstraction, it need only be pointed out that these are such only in the sense that the atom of physical science is an abstraction (not a fact of experience); and that in exactly the same way the conception of a cause is an abstraction, as every concept is and must be an abstraction since every concept is a formulation designed for the purpose of description and nothing else.

Concerning the nature and use of both the ultimate unit and the final unity dispute has arisen, that is, in regard to psychological atomism and the conception of the self; while the formulation and the application of the intermediate series of concepts is a matter of common agreement. But it is to be noted that these are all equally abstractions, the proximate as much as the final. We cannot say sensation is an abstraction but perception a concrete fact of experience, nor may we say association is an abstraction but the self a fact of immediate experience. If therefore any such conception as 'mental state' or 'mental element' is helpful in describing the phenomena of individual experience, it can be no argument against its use to say that it is impossible to point to such states as facts of experience.

Whether physical or mental the aim of science is to formulate a system of laws which shall represent the relations in which facts stand. It is for the purpose of making these connections intelligible and representable that all its concepts have been devised. In every branch scientific procedure depends upon two general classes of concepts, the one of which may be called constituent, the other unifying. The first is the product of analysis, the second of synthesis. The former process is strictly instrumental to the latter; for to define a thing in terms of an element is the necessary preliminary to its expression through a synthetic formula. In these two ways we deal with the content of experience whatever its qualitative character. This procedure has given us, on the one hand, factors, units or elements; and, on the other, relations, laws and systems. Both series of conceptions are indispensable. We conceive the object of reflection in terms of elements and the law of their combination. Each of these is an abstraction

from the intuition in which it is presented. They neither are interpolated in the series of experiences as facts among facts with which reflection is dealing, nor do they exist alongside the latter as an independent system. The content of every object of reflection is thus conceived in terms of certain constituent qualitative elements, and its form is described by means of a synthetic formula which expressed the relation of these elements.

The function of logical conception in the approach to problems of mental phenomena differs in no way from that which it fulfils in the physical and biological sciences. In psychology it is the system of facts of individual experience which is to be studied. To deal with these facts a series of conceptions is indispensable. These conceptions are necessarily abstract, that is, they are not facts of experience but descriptive formulas conceived for the purpose of expressing the relations of phenomena. In psychology, too, as in every science, the procedure of reflection involves those two phases which we call analysis and synthesis. The result is therefore necessarily some kind of element or constitutive unit representing the content, and some kind of law or unifying formula, representing their organization. Both of these terms are relative to the nature of facts involved, and represent all levels of complexity. By element, therefore, one does not mean the ultimate product of analysis but only its proximate outcome; nor by law the final unity which experience presents, but only the organic form of the facts in question. The unities achieved at a given level may themselves become the constituents of a higher unification, and the elements which analysis reveals on one occasion may themselves be subjected to analysis on another. Thus the contents of a sensation-complex constitute the elements which are unified in perception; the content of presentation, together with that of memory, provides those which are unified in apperception; the incident impression or idea and that with which it was previously juxtaposed afford the elements which are expressed in the law of contiguity; the successive events, those which are conceived in terms of causal connection; etc. Whether

it deal with any individual fact in the stream of consciousness, with the unity of any moment, with the permanent features of habit and judgment, or with the total complex which mental life presents, its point of view remains unchanged. Psychology can no more treat an individual fact of experience, as experience, than it can deal with the unity of individual consciousness as a whole. Each is to be expressed, first, in terms of its internal relations. It is conceived as a complex presenting a problem of structure, which is resolved in the concept of a unit of constitution and a formula of organization expressing this system of relations. Each experience, in the second place, is to be conceived as a unit having relations, of dependence, support and the like, with other elements and complexes of phenomena in a larger world, and therefore as presenting a problem of external relations. The determination of this system of relations and its expression in a series of laws is the second task of descriptive psychology. The individual experience is a complex whole, expressive of the functional relation of parts; but it is also a unit in a larger whole made possible through the existence of many individuals in those specific relations which characterize their organization. The self likewise is a complex system expressing in its form the relation of functionally connected elements; but it is also a unit in its reaction upon the external world and in its adaptation to other human wills.

Whether psychology deal with the self, then, or with the individual experience, it must treat its subject-matter as a system of phenomena, expressing its analysis in terms of a unit and its synthesis in terms of a law. In the proper sense it is not the self which is dealt with in any such case, but in the final analysis an individual event alone. The starting-point is the unit of real experience. This unit psychology conceives as a complex phenomenon whose constitution it must ascertain and formulate in a conceptual law. Out of these elementary abstractions is built up a series of concepts of increasing complexity or generality. These constitute the synthetic forms of wider and wider aspects of experience. The psychologist is bound to seek these forms throughout

his whole field, to treat of conception as well as intuition, of reasoning as well as judgment, of character as well as apperception, of adaptation as well as imagination. He must describe the stream of consciousness as a whole and indicate the most general characteristics of the mind just as he must analyze the content of each moment and establish the association-forms in which their elements appear.

The whole mental life is thus the field of psychology, in its grosser features and more permanent relations quite as much as in its minor details and individual characteristics. But while its province is coextensive with mental experience it is still a single continuous field within which only one type of material is to be found, namely mental facts and their relations. The existence of these facts in a single connected system does not constitute a self, if that term be used to describe the unity of the real subject. Psychology does not substitute for the known self a reality of the same order but corrected, rationalized, understood. There is but one unitary process of experience, upon which mental science makes simply an illuminating comment.

If these two meanings of self are to be excluded—if the psychologist is concerned neither with the assessment of immediate values in life nor with the metaphysical interpretation of experience—there remains the question, whether the term is to be excluded or retained and, in the latter case, what meaning is to be given to it. As commonly used it is not free from ambiguity, and within its more technical application there is a merely formal as well as materially important use of it.

In the first place there is a specific meaning which may be given to the term self in psychology as denoting something which the scientist is called upon to study among other particular mental facts. The sense of self is both a recurrent experience and an element of experience, though probably not universally or necessarily. As a recurrent event, while the sense of self-existence may not be an important constituent of experience it is nevertheless a familiar fact. At times, either for practical or theoretical reasons or as the result of

uncontrollable conditions, the self looms up in consciousness and engrosses attention. One is said to be *self-conscious*. The content and connections of such a sense of self vary greatly from occasion to occasion, but the nature of the phenomenon is not obscure and its analysis and explanation involve no conception which the general treatment of psychological data has not already formulated. In such cases there is commonly an interruption of the orderly flow of ideas, a disturbance of both the vasomotor and the voluntary muscle systems, and an intensifying of the affective tone of experience. The ordinary network of sensations, images and reactions is thus replaced by a new and characteristic complex; but while it constitutes a unique and frequently distressing consciousness its make-up as to nature differs in no way from those mental states at large with which the psychologist deals.

As an element of conscious experience at large the sense of self is introspectively discernible in at least its major part and, as a constituent of mental life, may extend beyond the human subject to include the range of brute existence. But in this sense the term means no more than a dim undefined background of feeling which characteristically accompanies presentative and representative experience alike, and resembles more perhaps than anything else the vague sense of familiarity which penetrates our common apprehension of the external world. It is a vital constituent of experience for each one of us, yet little is to be made of it theoretically; it is there, it is to be noted, and that is all.

In the next place the term self has a material application in psychology as indicating the totality of mental characteristics and activities with which that science is concerned. Its field is the mental life, in all its types and modifications, normal and pathological, developmental and degenerative. This system of data,—the field of experience as subjective and individual—reveals, like that of any natural science, an unsuspected complexity of structure and relationship as it is explored. Even the normal self presents a bewildering profusion of idiosyncrasies in individual habit and reaction, in

qualitative make-up and affective tone, under specific stimulation.

When from its original terms in the normal adult self one traces the expansion of psychological interest in every direction, which has taken place in the past generation, and notes even the mere quantitative addition to our knowledge of mental phenomena, the largeness of the task is first dimly apprehended. But one thing at least is forced upon the attention, that in its actual inner variety and in its subtleties of response or rapport the features of the human mind are no more made known in the ordinary course of experience than is the subject-matter of any other science to those who may have to deal with its materials in a practical way.

The work of psychology in all its branches, experimental and theoretical, is directed to the development of that complex conception which reflects the rich and varied life of the mind. This life, complicated beyond representation in its characteristics and activities, subject to modification in its specific features by every incident stimulus and undergoing progressive development with each successive experience, and therefore explicable only in terms of its whole history—this self, as we call it, can no more be given to the subject in an intuition, that is, be realized as an experienced fact, than it can be given to an objective observer. It is presented in any moment's experience only in a certain phase, relation or attitude. It can be known only through the whole succession of experiences which constitutes individual history, for its features are not given a priori but must be learned empirically by the subject as by all others. In its fulness it is the summation of all those individual characteristics which are determined through reaction to specific situations, the occurrence of which depends upon the order of the objective world.

The filling in of this picture is, in general, the task of descriptive psychology, and its subject is commonly called the Self. This empirical application of the term is grounded in popular speech and its use in this connection involves no necessary confusion. Nevertheless the term is not free from

ambiguity—since it is also employed with those other meanings already pointed out—and the psychologist has at his command, in the word 'mind,' another common term which is free from these secondary implications. Especially when the field is extended, in comparative psychology, to include lower forms of life it becomes increasingly apparent that the term 'self' does not express with exactness what the psychologist intends. It is because the term 'mind' carries just this specific reference to the complex of thoughts, feelings and actions which individual experience presents, that it has obtained a secure place in psychological terminology as designating its general field of exploration.

Contrasted with this use is another application of the term 'self' which expresses nothing but the logical limit of reference postulated in the definition of the science itself. The intuitional content of the term has already been discriminated from its scientific application. In the former case we mean by the self the actual subject of experience. When I say: the self is real, I mean thereby to assert only what is implied in the statement: I am. In this sense the self is neither phenomenal nor noumenal, for each of these terms implies a logical resolution of the manifold of experience. Or, it might perhaps be said with equal truth, it is both noumenal and phenomenal, since each of these treatments of reality, if valid, must be true of the self. It has a phenomenal existence, the content of which psychology treats; and it has a noumenal significance with which metaphysics deals. Though neither science nor metaphysics is concerned with the subject of immediate experience, it is the necessary object of reference in all treatment. In psychology it becomes necessary to mark the unity of the whole system of phenomena with which the science deals. In the first place no mental activity exists independently; correlation is everywhere presented. The various mental processes mutually condition, modify and contribute to one another; they form a network of connected functions. In the second place through all the data of psychology runs a qualitative identity which may be characterized by saying that everything the psychologist touches must be

conceived in terms of individual subjective experience; it must either be itself a fact of consciousness or be restatable as a condition or product of consciousness.

Now the term self has been used to denote this unity of materials and relations which must everywhere be assumed but nowhere becomes the object of descriptive treatment. It marks the field within which every inquiry falls. All psychology is the psychology of self, but there is no psychology of selves which may be placed over against what may be called the psychology of the constituents of the self, that is, of the particular individual phenomena of the mental life. For everything which science treats must be conceived in terms of its relations to other things; and while each of the special mental processes exists in a system of functions and thus affords a field for psychological treatment, the same does not hold true of the self.

It cannot be urged that the self must be susceptible to treatment as a unit in a larger system of things since social psychology exists. The latter can be differentiated in no way from individual psychology as regards either the nature of its subject-matter or its general conceptions. It investigates a special group of activities and products falling within the general field of psychological inquiry, namely those which depend upon the fact that man lives in association with his fellows and that this mode of life gives rise to certain communities of thought, feeling and action which do not come into sight so long as one regards the psychological subject from a strictly individual standpoint. But these modifications arise like all others. They appear as reactions upon changes in the external world and introduce no new order of occurrences into the mental life. The only object which is ever before the psychologist is thus some special mental event or process, and his problem invariably is the determination of this activity in relation to the system within which it is found through the establishment of its connections with other individual processes.

ON THE INFLUENCE OF PREVIOUS EXPERIENCE ON PERSONAL EQUATION AND STEADINESS OF JUDGMENT IN THE ESTIMATION OF THE NUMBER OF OBJECTS IN MODERATELY LARGE SAMPLES

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I. Introductory Remarks

In much of the work in which personal equation is a factor the observer is not able to compare his estimates with the true value, and so attempt at each successive observation to profit by his previous experience. If experiments be arranged in which this can be done, the influence of experience upon both personal equation and steadiness of judgment can be studied.

Highly satisfactory materials for such a study are furnished by the data of an earlier paper in which I have shown that there is a pronounced personal equation in the estimation of the numbers of objects in moderately large samples, and have given measures of personal equation and of steadiness of judgment based on the massed statistics of twenty-eight experiments, comprising altogether 15,200 estimates.[1]

In these experiments the errors of observation were recorded in sequence. A group of fifty consecutive estimates with the accompanying determinations of the errors constituted a 'period.' With the exception of a single experiment in which the number of objects was so large that the amount of time required for the counting rendered more than 50 estimates per day undesirable, record sheets for a morning and an afternoon period were filled by each observer.

[1] Harris, J. Arthur, 'Experimental Data on Errors of Judgment in the Estimation of the Number of Objects in Moderately Large Samples, with Special Reference to Personal Equation,' this journal, November 1915, 22, pp. 490–511.

Each 'period' is for convenience divided into five 'trials' of ten successive estimates each.

In making these estimates each observer made a persistent effort to improve. This was based on a knowledge of the immediately preceding errors and consisted in a constant effort to lay out the desired number of seeds. The constants from the distributions of errors made in the individual experiments were calculated from time to time in the same laboratory, so that the observers had some knowledge of the results of preceding experiments taken as a whole. This knowledge did not, I am quite sure, have any influence upon subsequent experiments.

The main problems involved in the question of experience are two: Is there a change in personal bias as a result of constant effort to improve and opportunity for improvement? Does the judgment become steadier, *i. e.*, does the observer make less erratic estimates, as a result of experience?

Both of these questions are really twofold. Is there an improvement from period to period? Is there an improvement within the period? In short, does the worker improve both from estimate to estimate in the same half daily period and also from period to period?

II. Analysis of Data

Consider first the problem of improvement from period to period.

To test the matter most simply one may merely split an experiment, say our first 700 estimates, into the first and second half, and determine the difference between the constants of the first and second 350 estimates. For personal equation the constants are:

Observer	First 350	Second 350	Difference
Observer *B*..........	$+ .120 \pm .164$	$+ .223 \pm .136$	$+ .103 \pm .213$
Observer *C*..........	$+1.331 \pm .159$	$+ .520 \pm .125$	$- .811 \pm .202$
Observer *D*..........	$+2.691 \pm .210$	$+1.649 \pm .160$	$-1.043 \pm .264$

Observer *C*'s and observer *D*'s personal equations have dropped by an amount four times their probable errors in

passing from the first half to the second half of the experiment. Observer *B*'s has increased by an amount only half its probable error.[1]

For steadiness of judgment the results are:

	First 350	Second 350	Difference
Observer *B*.........	4.552±.116	3.771±.096	− .781±.151
Observer *C*..........	4.408±.112	3.474±.089	− .934±.143
Observer *D*..........	5.837±.149	4.438±.113	−1.399±.187

In all cases there is a distinct and statistically significant decrease in the standard deviation—the judgments becoming less erratic as experience becomes greater.

Instead of contenting oneself with so crude a method as a comparison of the constants for the two halves of an experiment, one may obtain a quantitative expression for the influence of experience by correlating between the number of previous experiences and the measures of personal equation or of steadiness of judgment.

In doing this one must deal with a number of subgroups for each period. It is most convenient to divide each half daily period of 50 estimates into five consecutive 'trials,' each of 10 estimates. For each of these 'trials' the mean personal equation and the standard deviation of the errors must be computed. Thus in obtaining the constants presented here it was first necessary to compute 1,520 means and 1,520 standard deviations, which were then treated as units in computing the correlations.

Since the constant desired is the correlation between the number of previous experiences (or of 'trials') and personal equation and steadiness of judgment, the first period (or 'trial') must be designated as 0 (no previous experience) and the subsequent ones numbered consecutively, beginning with 1.

I consider first the results for the correlation between the number of previous periods of experience and personal equation.

[1] The logical conclusion seems to be that Observer *B*, who at the beginning had practically no personal equation has not changed throughout the work, while Observer *C* and Observer *D* who had distinct personal equations at the beginning have been able to reduce theirs as the result of their experience.

Table I[1] gives the correlation coefficients, r_{pe} with their probable errors and the ratios to their probable errors. The results may also be expressed in terms of the regression of personal equation on periods of previous experience as shown by the straight line equations in Table II., calculated from the formula

$$e = \left(\bar{e} - r_{pe}\frac{\sigma_e}{\sigma_p}\bar{p}\right) + r_{pe}\frac{\sigma_e}{\sigma_p}p,$$

TABLE I

PERIODS OF EXPERIENCE AND PERSONAL EQUATION

Experiment	Observer *B*	r/E_r	Observer *C*	r/E_r	Observer *D*	r/E_r
I.	−.053±.080	−0.66	−.237±.076	−3.12	−.189±.078	−2.42
II.	−.161±.085	−1.89	−.108±.086	−1.25	−.433±.071	−6.10
III.	−.344±.077	−4.47	−.050±.087	−0.57	−.388±.074	−5.24
II.+III.	−.122±.060	−2.03	−.186±.059	−3.15	−.369±.053	−6.96
IV.	+.002±.087	+0.02	+.260±.081	+3.21	−.073±.087	−0.84
V.	−.025±.087	−0.29	−.057±.087	− .66	+.037±.087	+0.43
IV.+V.	−.089±.061	−1.46	+.027±.061	+ .44	+.144±.060	+2.40
VI.	−.017±.151	−0.11	+.353±.132	+2.67	+.078±.150	+0.52
VII.	....	.....	−.251±.094	−2.67	−.588±.066	−8.91
VIII.	....	.....	....	.....	+.114±.086	+1.33
IX.	−.440±.073	−6.02	−.524±.066	−7.94	....	.....
X.	+.070±.100	+0.70	−.230±.095	−2.42	....	.....
IX.+X.	−.307±.061	−5.03	−.034±.061	− .55	....	.....
XI.	−.280±.080	−3.50	−.006±.087	−0.07	+.275±.080	+3.44

TABLE II

REGRESSION OF PERSONAL EQUATION ON PERIODS OF PREVIOUS EXPERIENCE

Experiment	Observer *B*	Observer *C*	Observer *D*
I.	$e=+0.350-.028\,p$	$e=+1.441-.079\,p$	$e=+2.741-.088\,p$
II.	$e=+0.863-.064\,p$	$e=+1.279-.046\,p$	$e=+2.099-.274\,p$
III.	$e=+1.159-.118\,p$	$e=+0.655-.017\,p$	$e=+1.234-.291\,p$
II.+III.	$e=+0.771-.023\,p$	$e=+1.217-.037\,p$	$e=+1.611-.130\,p$
IV.	$e=+0.778+.001\,p$	$e=+0.096+.172\,p$	$e=+0.182-.072\,p$
V.	$e=+0.366-.016\,p$	$e=+1.063-.031\,p$	$e=+0.740+.027\,p$
IV.+V.	$e=+0.917-.033\,p$	$e=+0.874+.008\,p$	$e=-0.391+.063\,p$
VI.	$e=+0.904-.066\,p$	$e=+0.082+1.252\,p$	$e=+2.404+.434\,p$
VII.	....	$e=+0.623-.086\,p$	$e=+2.194-.361\,p$
VIII.	....	....	$e=+0.011+.035\,p$
IX.	$e=+1.450-.129\,p$	$e=+0.737-.159\,p$	....
X.	$e=+0.348+.021\,p$	$e=+0.411-.061\,p$	....
IX.+X.	$e=+1.085-.047\,p$	$e=+0.090-.005\,p$	....
XI.	$e=+0.791-.111\,p$	$e=+0.232-.002\,p$	$e=-0.617+.174\,p$

[1] On general principles I feel that it is highly desirable to publish the original data from which all statistical constants are deduced. In the present case, however, it seems quite out of the question to print the 128 correlation tables upon which the conclusions of this paper are based.

where e = personal equation, p = periods of previous experience, the bars denote the means of the two characters and the sigmas their standard deviations. In these equations

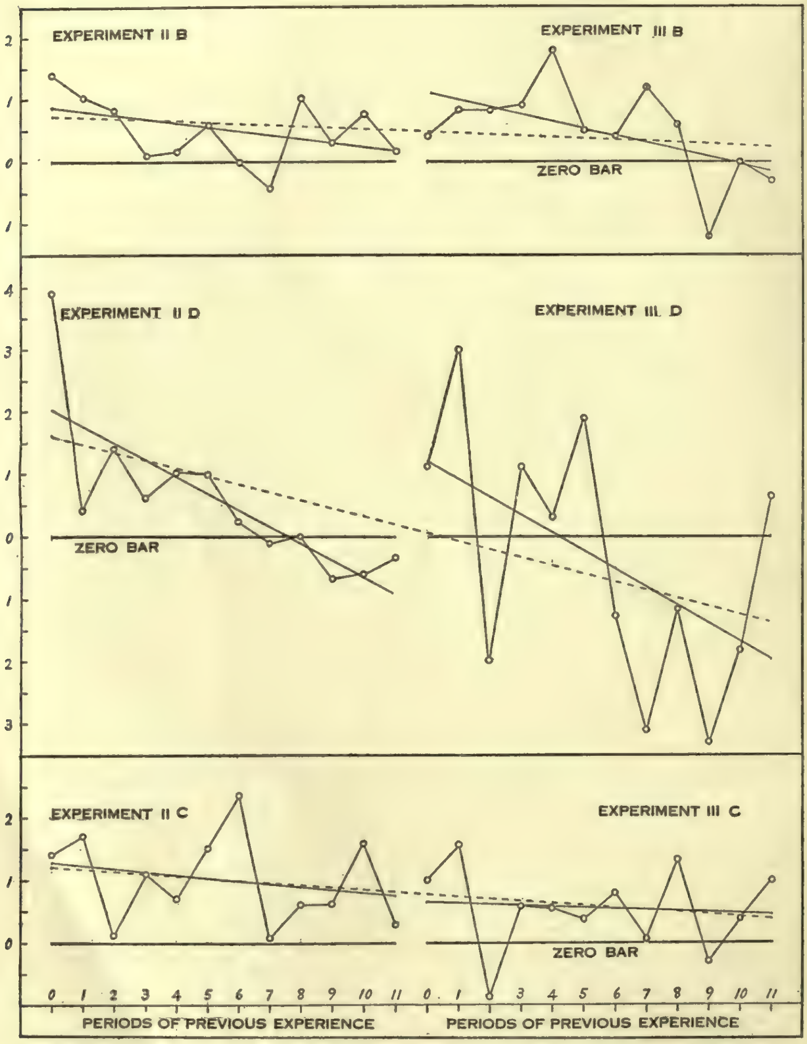

DIAGRAM I. Mean Personal Equation for the Successive Periods of an Experiment. The position of the circles indicates on the scale to the left the mean personal equation for the period. The solid lines represent the regression equations for the individual experiments. The broken lines represent the straight line equations for the combinations of the two successive experiments.

the second term shows the actual amount of change in personal equation per period of previous experience.

The empirical means (personal equations) for each period

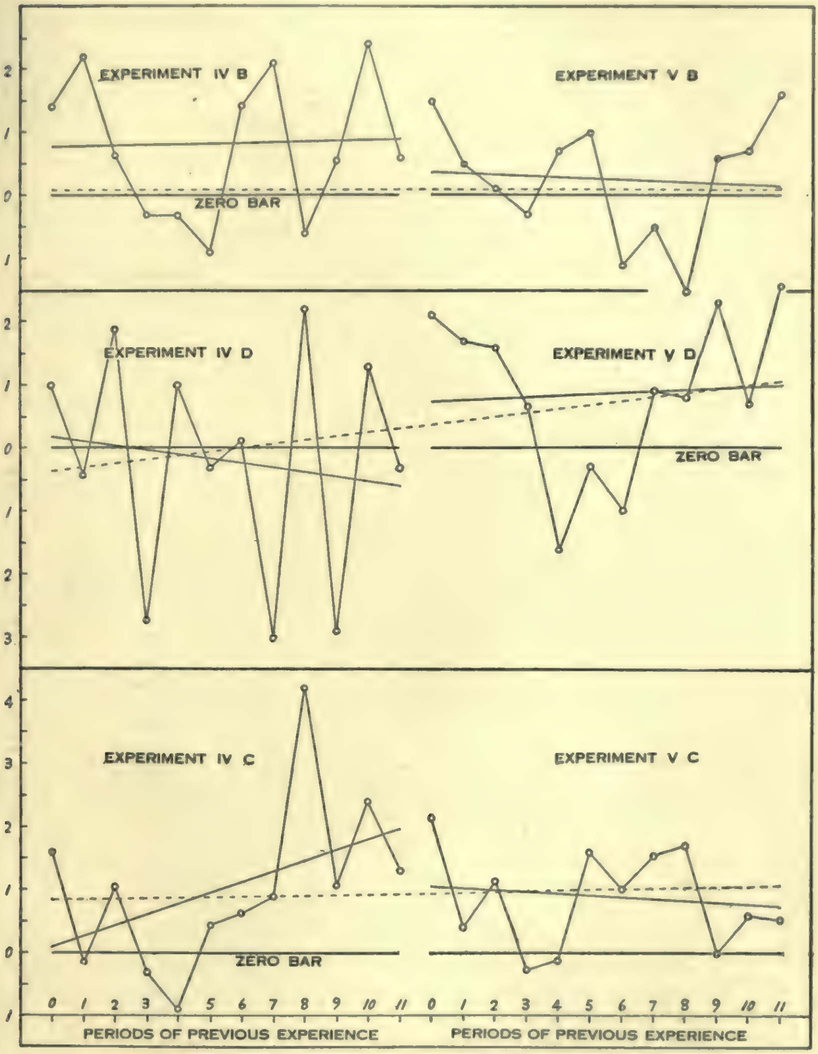

DIAGRAM 2. Explanation as in Diagram 1. Note that personal equation sometimes increases and sometimes decreases with experience. Note that in both cases the means are very irregular but that there is a great preponderance of those on the positive side of the zero bar—*i. e.*, of cases in which the persistence of personal equation is shown in the individual periods.

have already been shown graphically for the first experiment in Fig. 4 of the preceding paper. The lines superimposed upon these are those representing the regression straight line equations. The lines for twelve additional series are shown in Fig. 1-2. These are for the experiments belonging to sets II.-III. and IV.-V., which were made in consecutive weeks. Hence they are so drawn that the regression line (dotted) for the combined experiment may be drawn through the same set of empirical means.

These diagrams may suggest that personal equation tends to become somewhat less pronounced with experience. There are, however, conspicuous exceptions. They show great irregularity in the distribution of the means of the personal equation of individual periods. There is no evidence that taken as a whole these irregular means could be better represented by a curve of a higher order than by a straight line.

The diagrams further show not only that the individuals differ in their mean personal equation from period to period, but that in the case of consecutive experiments the whole trend of the experiments may differ sensibly.

Turning to the numerical values as a means to finer analysis, it appears that in 20 cases the correlation is negative as compared with 8 cases in which it is positive in sign. Of the 8 correlations involving the data of two consecutive experiments 6 are negative and 2 are positive in sign. Experience tends generally, therefore, to change personal equation in the negative direction. Since the personal equations of the observers who took part in these experiments were for the most part of the positive sort, experience leads on the whole to an improvement of the estimates in so far as personal equation is concerned.

In the case of Experiment *C* IX. both personal equation and correlation are negative. The tendency is therefore for the observer to become worse by experience. The same is true in the case of *D* III. and *D* IV.

Thus in so far as the constants may be classified by the signs of the personal equations and of the correlations, there are $8+3 = 11$ cases in which the personal equation tends to

grow worse with experience to 20 − 3 = 17 cases in which it tends to become better. This means that if one calculated the personal equation at the *beginning* and at the *end* of the whole experiment from a straight line equation fitted to the means *of the whole experiment* he would find that in 17 cases the calculated personal equation at the end of the periods of experience would be nearer zero than that at the beginning, whereas in 11 cases the reverse would be true. This point may be easily verified from the equations given.

Averaging the constants for the three individual observers I find, regarding signs,

	Mean Correlation.
Observer *B*	−.136
Observer *C*	−.085
Observer *D*	−.130

For the whole 28 experiments the results are:

	Mean Correlation.
Eight positive coefficients	+.151
Twenty negative coefficients	−.223
All twenty-eight determinations	−.116

If one considers that the three cases in which a negative correlation is associated with a negative personal equation should be classed with the cases in which personal equation tends to become larger with experience the results are:

For 17 cases with tendency to decrease personal equation	−.204
For 11 cases with tendency to increase personal equation	+.199

An examination of these coefficients in their relation to their probable error shows that only 14 out of the 28 are at least twice as large as their criterion of trustworthiness. Of these, 3 are positive and 11 are negative. In two of these cases, however, *i. e.*, *C* IX. and *D* III., the negative sign of the correlation coefficient indicates an increase in the numerical magnitude of personal equation as a result of experience. Hence the statistically significant coefficients—where by statistically significant one means a coefficient at least twice as large as its probable error—stand in the ratio of 11 − 2 = 9 to 3 + 2 = 5 in evidence of a modification in the direction of betterment of personal equation by experience. Thus taken as a whole these data perhaps show a slight but distinct

tendency for the observers to reduce their personal equation by a persistent effort to improve and the constant opportunity for improvement afforded by a knowledge of the amount of error of each estimate made. Improvement by modification of personal equation is however, if it exists at all, very slight.

These findings are in good accord with the results for personal equation presented in the preceding paper, *i. e.*, the demonstration of its persistence in each of the three observers throughout a period of two years.

Turn now from the question of improvement as a result of experience from period to period to the problem of improvement within the period.

Each period of 50 estimates has been split up into 5 trials of 10 errors each, and the personal equation (*i. e.*, the mean deviation) and the standard deviation for each of these computed. These trials are now classified not by the sequence of the periods of experimentation but by first to fifth trial within the periods. In determining the correlation between the number of previous trials and personal equation the first must be ranked as 0 (no previous trials) and the subsequent ones as 1 to 4.

TABLE III

TRIALS WITHIN THE PERIOD AND PERSONAL EQUATION

Experiment	Observer B	r/E_r	Observer C	r/E_r	Observer D	r/E_r
I.	$+.370\pm.070$	$+5.29$	$-.073\pm.080$	-0.90	$-.002\pm.081$	-0.03
II.	$+.214\pm.083$	$+2.58$	$+.234\pm.082$	$+2.85$	$-.329\pm.078$	-4.21
III.	$+.193\pm.084$	$+2.30$	$-.074\pm.087$	-0.85	$-.150\pm.085$	-1.76
IV.	$+.252\pm.082$	$+3.07$	$-.103\pm.086$	-1.20	$-.062\pm.087$	-0.71
V.	$+.160\pm.085$	$+1.88$	$-.229\pm.083$	-2.76	$+.207\pm.083$	$+2.49$
VI.	$-.093\pm.150$	-0.62	$-.088\pm.150$	-0.59	$-.233\pm.143$	-1.63
VII.	....	.....	$+.110\pm.099$	$+1.11$	$-.240\pm.095$	-2.53
VIII.	....	.....	....	.....	$+.377\pm.075$	$+5.03$
IX.	$+.140\pm.089$	$+1.57$	$+.001\pm.091$	$+0.01$	....	.....
X.	$-.191\pm.097$	-1.97	$-.025\pm.101$	$-.25$	....	.....
XI.	$+.170\pm.085$	$+2.00$	$-.194\pm.084$	-2.31	$-.111\pm.086$	-1.29

The results are given in Table III. Of the 28 constants 12 are positive and 16 negative in sign. In all cases in which personal equation is positive the negative correlations may be looked upon as indicating a tendency to improvement, whereas in all cases in which personal equation is negative the positive correlations may be taken to indicate improvement. Judged

by this standard two of the negative coefficients *D*, III. and *D* IV., indicate (if they may be considered statistically trustworthy) a tendency to become worse with experience. In the third case of negative personal equation, *C* IX., the correlation is positive in sign.[1] Hence, viewed from the standpoint of progress in the direction of the elimination of personal equations as the result of successive trials of ten attempts each within the individual experimentation periods the results stand 15 : 13, in favor of an improvement within the period.

Only 11 of the 28 constants are two or more times as large as their probable errors. Of these 8 are positive and 3 are negative in sign.

The averages, regarding signs, are:

	Mean Correlation.
Observer *B*	+.135
Observer *C*	−.034
Observer *D*	−.060

The mean of the 12 positive coefficients is + .202, that of the 16 negative ones − .131, that for all the experiments + .012.

The results, extensive as they are, indicate very clearly by the low magnitude of the correlations and their nearly equal distribution into positive and negative values that there is no appreciable change in personal equation as a result of an effort to improve within a period of 50 trials.

I now turn to a consideration of the relationship between the number of periods of previous experience and steadiness of judgment as measured by its standard deviation. The correlation coefficients with the means of estimating their statistical trustworthiness are given in Table IV. The evaluations of

$$s = \left(\bar{s} - r_{ps}\frac{\sigma_s}{\sigma_p}\bar{p}\right) + r_{ps}\frac{\sigma_s}{\sigma_p}p,$$

where s = steadiness of judgment as measured by the standard deviation and the other symbols have their usual significance, are given in Table V.

[1] Neither of the three constants, *D* III., *D* IV., nor *C* IX., can safely be regarded as trustworthy in comparison with its probable error.

The straight lines for the first experiment covering 14 periods are shown with their empirical means in Diagram 3. The means and fitted lines for the two sets of experiments which were made in consecutive weeks are shown in Diagrams 4–5.

Table IV

For Periods of Experience and Steadiness of Judgment

Experiment	Observer *B*	r/E_r	Observer *C*	r/E_r	Observer *D*	r/E_r
I.	$-.457\pm.064$	-7.14	$-.347\pm.071$	-4.89	$-.474\pm.063$	-7.52
II.	$-.305\pm.079$	-3.86	$-.263\pm.081$	-3.25	$-.434\pm.071$	-6.11
III.	$-.074\pm.087$	$-.85$	$-.013\pm.087$	-0.15	$+.022\pm.087$	$+.25$
II.+III.	$-.420\pm.051$	-8.28	$-.263\pm.057$	-4.61	$-.319\pm.055$	-5.80
IV.	$-.340\pm.077$	-4.42	$-.290\pm.080$	-3.63	$-.240\pm.082$	-2.93
V.	$-.068\pm.087$	-0.78	$-.411\pm.072$	-5.71	$-.084\pm.087$	$-.97$
IV.+V.	$-.431\pm.050$	-8.62	$-.468\pm.048$	-9.75	$-.265\pm.057$	-4.65
VI.	$-.337\pm.134$	-2.51	$-.311\pm.136$	-2.29	$-.628\pm.091$	-6.90
VII.	....	.....	$-.258\pm.094$	-2.74	$-.451\pm.080$	-5.64
VIII.	....	.....	....	.....	$-.037\pm.087$	$-.43$
IX.	$-.244\pm.086$	-2.84	$-.095\pm.090$	-1.06	....	.....
X.	$+.220\pm.096$	$+2.29$	$-.244\pm.095$	-2.57	....	.....
IX.+X.	$-.284\pm.062$	-4.58	$-.348\pm.059$	-5.90	....	.....
XI.	$-.086\pm.086$	-1.00	$-.160\pm.085$	-1.88	$-.247\pm.082$	-3.01

Table V

Regression of Standard Deviation Measuring Steadiness of Judgment on Periods of Previous Experience

Experiment	Observer *B*	Observer *C*	Observer *D*
I.	$s=4.190-.106p$	$s=4.350-.087p$	$s=5.770-.175p$
II.	$s=3.577-.076p$	$s=3.743-.078p$	$s=4.480-.157p$
III.	$s=2.609-.014p$	$s=2.927-.003p$	$s=3.069+.006p$
II.+III.	$s=3.424-.050p$	$s=3.518-.035p$	$s=3.948-.051p$
IV.	$s=6.784-.140p$	$s=7.402-.164p$	$s=7.325-.170p$
V.	$s=5.027-.025p$	$s=6.059-.153p$	$s=5.721-.039p$
IV.+V.	$s=6.500-.091p$	$s=7.237-.120p$	$s=6.883-.081p$
VI.	$s=14.755-.959p$	$s=13.808-.951p$	$s=17.770-2.278p$
VII.	....	$s=1.836-.039p$	$s=3.109-.161p$
VIII.	....	....	$s=2.008-.006p$
IX.	$s=2.275-.038p$	$s=1.817-.010p$	....
X.	$s=1.666+.035p$	$s=1.663-.034p$	....
IX.+X.	$s=2.184-.024p$	$s=1.872-.023p$	....
XI.	$s=2.959-.019p$	$s=3.010-.037p$	$s=4.565-.079p$

In all of these diagrams the straight lines are fitted to the mean standard deviations for the individual trials of which 5, comprising 10 observations each, constitute a period. These means are represented by circles, connected by solid lines. As a check, and to show the influence of differentiation among

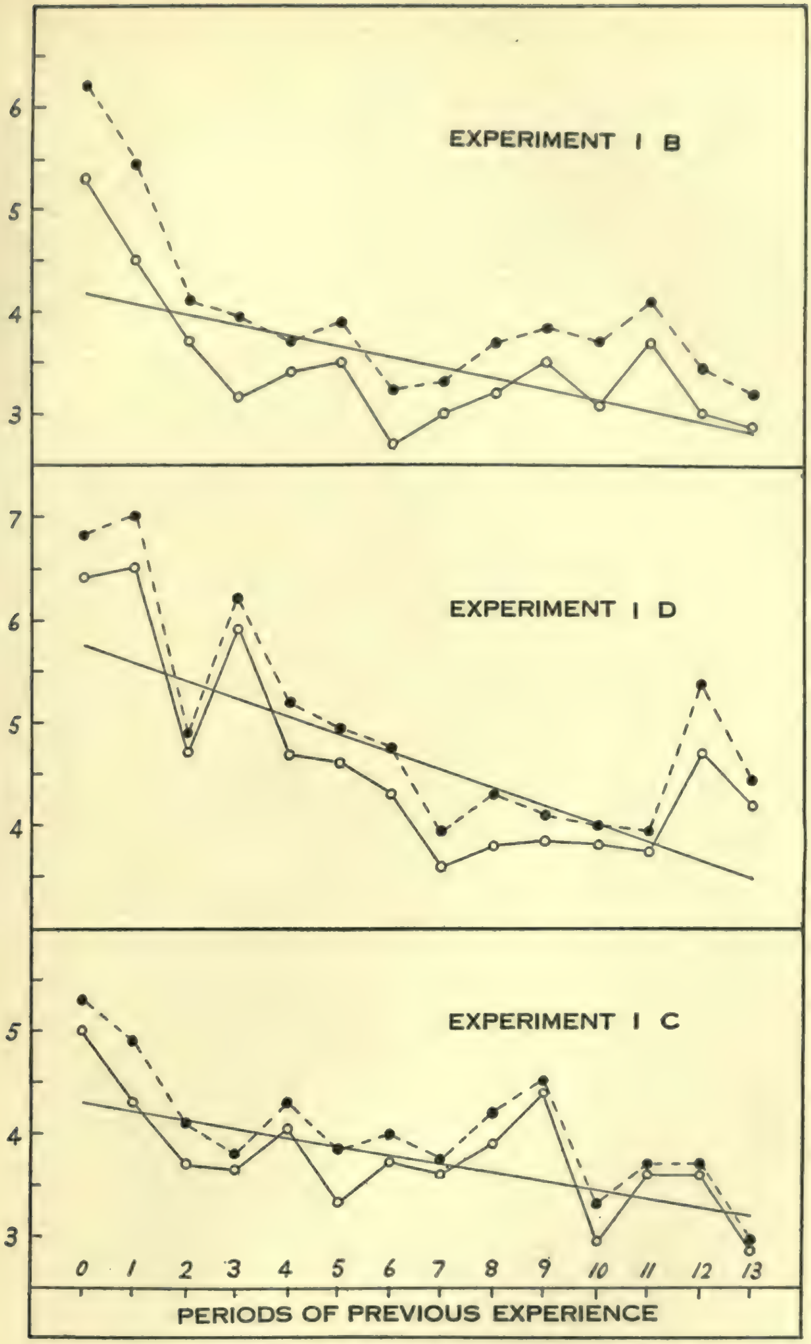

Diagram 3. Means of Five Standard Deviations in the Fourteen Periods of the First Experiments, Circles and Solid Lines, with the Fitted Regression Straight Lines. The solid dots connected by broken lines show the standard deviations for the fifty errors in each period.

the individual trials of a period the standard deviations of the 50 estimates of each period are shown by solid dots connected by broken lines in the first two of these diagrams.

In all cases the constant based on the 50 observations is higher than the mean of that of the 5 sub-groups of 10 constituting the period. This is to be expected from the fact that

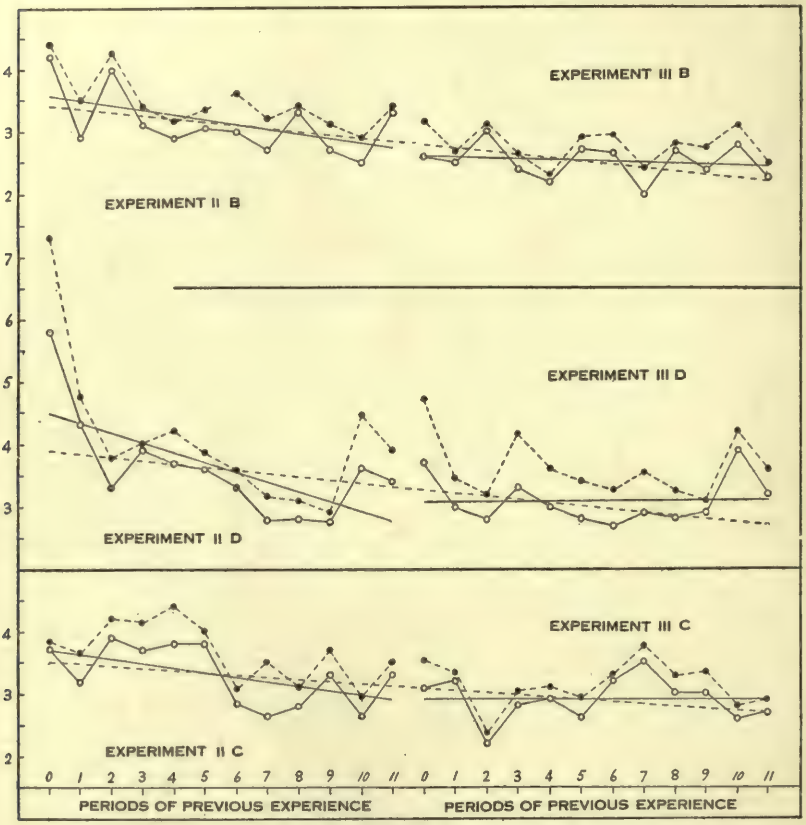

DIAGRAM 4. Explanation as in Diagram 3. The dotted regression lines show the rate of change, if considered linear, for the two consecutive experiments.

the individual trials of a period differ somewhat among themselves. These differences naturally contribute to the period standard deviation when the data of all the trials are thrown together.

These diagrams show something of the amount of change

occurring in steadiness of judgment as a consequence of previous experience. There is perhaps a slight suggestion of non-linearity. At first the standard deviation of the estimates

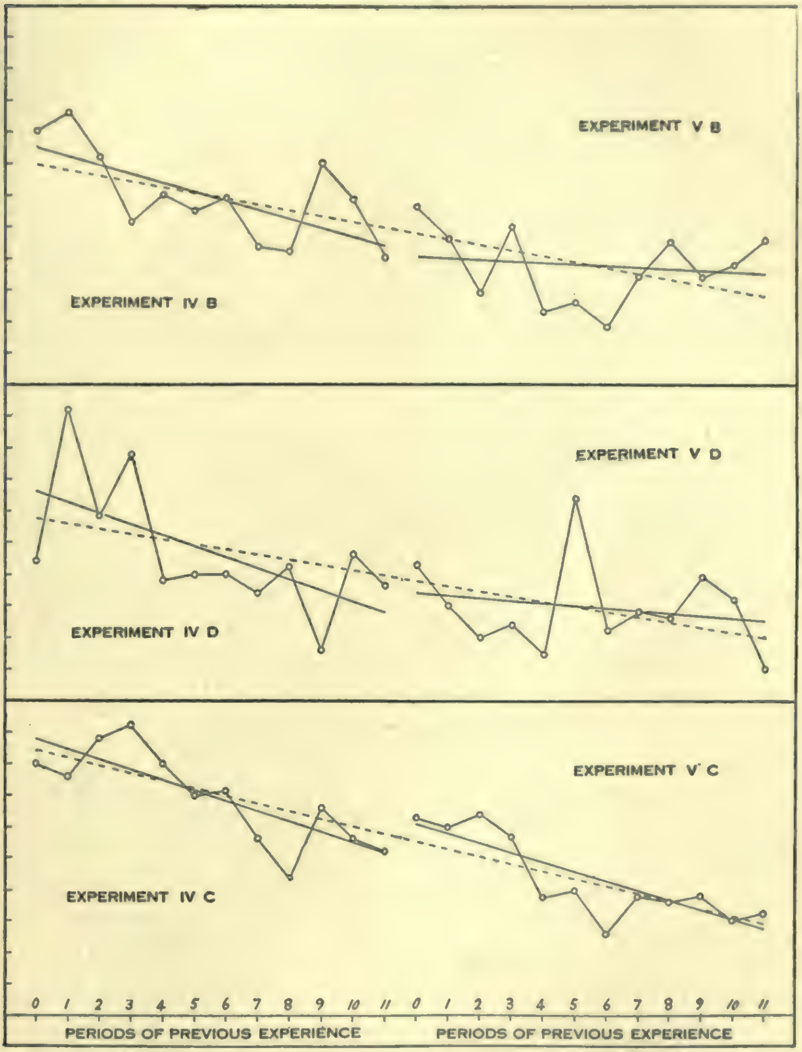

DIAGRAM 5. Explanation as in Diagrams 3–4. The Mean Standard Deviations only are shown.

perhaps falls slightly more rapidly than later. There are, however, cases in which this is not at all apparent. Con-

sidering the irregularity in the empirical means the equation to a straight line represents the change in steadiness of judgment fairly well.

From the table of correlation constants, it appears that *26 out of 28 coefficients are negative in sign.* Thus while personal equation decreases very little with experience as shown by the fact that both positive and negative correlations are found in not widely different numbers, steadiness of judgment unquestionably becomes greater as is indicated by the fact that in practically every experiment the mean standard deviation of the estimates made in the trials of the individual periods decreased as experience increased.

The same point is shown by an examination of the constants in comparison with their probable errors. In the case of the correlation between periods of previous experience and personal equation only 9 constants indicating an improvement in personal equations were at least twice as large as their probable errors. In the relationship here under consideration no less than 18 constants must by the same standards be considered statistically trustworthy. All eight of the correlations covering the range of previous experience afforded by a two weeks' test are *at least* 4.5 times as large as their probable errors, and so unquestionably show an improvement in steadiness of judgment.

The mean correlation for the individual observers are:

	Mean Correlation.
Observer *B*	−.188
Observer *C*	−.239
Observer *D*	−.286

For all observers the averages are:

	Mean Correlation.
Positive values, 2	+.131
Negative values, 26	−.265
All values, 28	−.238

From the problem of the relationship between the number of periods of previous experience and steadiness of judgment one naturally turns to the question of improvement within the individual periods, irrespective of their temporal position in the experiment. Table VI gives the correlation between the

number of trials, each of 10 estimates, and the standard deviation of the errors in the individual trials.

Table VI

TRIALS WITHIN THE PERIOD AND STEADINESS OF JUDGMENT

Experiment	Observer *B*	r/E_r	Observer *C*	r/E_r	Observer *D*	r/E_r
I.	$-.210\pm.077$	-2.73	$-.253\pm.076$	-3.33	$-.349\pm.071$	-4.92
II.	$-.109\pm.086$	-1.27	$-.422\pm.072$	-5.86	$-.128\pm.086$	-1.49
III.	$-.176\pm.084$	-2.10	$-.450\pm.069$	-6.52	$+.060\pm.087$	$+0.69$
IV.	$-.319\pm.078$	-4.09	$-.408\pm.073$	-5.59	$-.005\pm.087$	-0.06
V.	$-.234\pm.082$	-2.85	$-.294\pm.080$	-3.68	$+.013\pm.087$	$+0.15$
VI.	$-.051\pm.150$	-0.34	$-.566\pm.103$	-5.50	$-.324\pm.135$	-2.40
VII.	....	.....	$+.219\pm.096$	-2.28	$-.308\pm.091$	-3.38
VIII.	....	.....	....	.....	$-.032\pm.087$	-0.37
IX.	$-.159\pm.089$	-1.79	$-.473\pm.071$	-6.66	....	.....
X.	$-.036\pm.100$	-0.36	$-.276\pm.093$	-2.97	....	.....
XI.	$-.125\pm.086$	-1.45	$-.385\pm.074$	-5.20	$-.013\pm.087$	-0.15

The predominance of negative signs indicates that within the individual periods there is a decrease in the scatter of estimates, or an *increase* in steadiness of judgment. Of the 28 constants all but 3 have the negative sign. The averages are:

	Mean Correlation
Observer *B*..	$-.158$
Observer *C*..	$-.331$
Observer *D*..	$-.121$
All observers..	$-.208$

Of the 28 coefficients 17 are at least twice as large as their probable errors. One of these is positive, the remainder are negative in sign.

For the present it has hardly seemed worth while to calculate the regression equations for this entire series of relationships. For experiment I. they are:

	Regression Straight Line Equations
Observer *B*................................	$s=3.779-.138\,t$
Observer *C*................................	$s=4.144-.181\,t$
Observer *D*................................	$s=5.367-.367\,t$

Diagram 6 shows clearly how great the decrease in scatter of estimates as the result of a few 'trials' is. Apparently too the decrease cannot be satisfactorily represented by the slope of a straight line. The improvement is apparently more rapid at first than later, and some curves of a higher order are required to describe the rate of change.

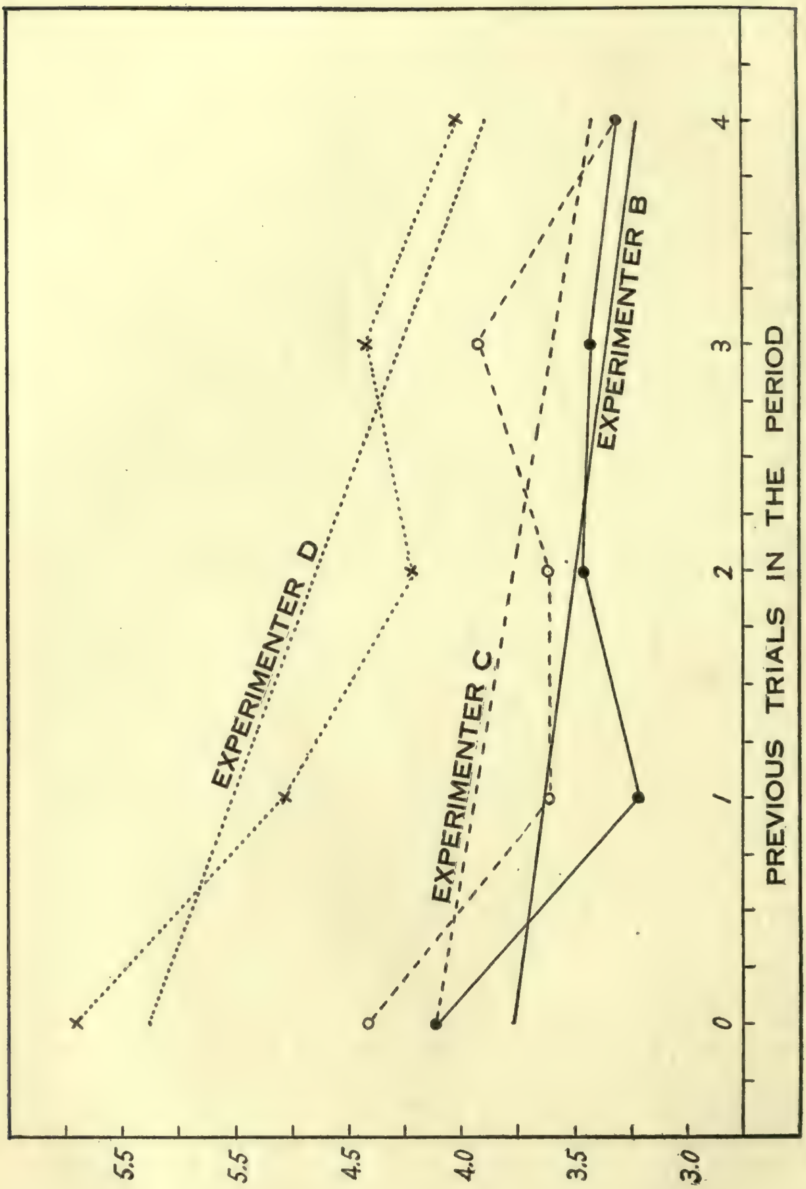

DIAGRAM 6. Mean Standard Deviations and Regression Straight Lines Showing Change in Steadiness of Judgment within the Individual Periods of Experiment I.

To carry this matter one step farther I have calculated the correlation ratios, η, as well as r. They are:

	Coefficient of Correlation	Correlation Ratio
Observer *B*	−.210	.337
Observer *C*	−.253	.364
Observer *D*	−.349	.398

Both of these constants for all of the three observers agree in indicating a real interdependence between the number of previous trials in a period and steadiness of judgment. The signs of the coefficients of correlation show that the estimates become less erratic, or the judgment steadier. The correlation ratio is necessarily positive. Applying Blakeman's test for linearity of regression[1],

	ζ / E_ζ
For Observer *B*	1.75
For Observer *C*	1.74
For Observer *D*	1.88

Thus these mathematical tests furnish no *proof* that the change in steadiness of judgment as the result of experience within the period can be regarded as other than linear. Sometime a detailed investigation of this question for the whole material may be in order, but there seems no advantage in going into it in greater detail at present.

III. Concluding Remarks

The purpose of this paper is the presentation of the results of a statistical analysis of extensive series of experimental data on the influence of previous experience upon errors of judgment in the estimation of the numbers of objects in moderately large samples.

The experiments comprised from 200 to 700 estimates each. These have been grouped for purposes of calculation into ultimate units of ten consecutive estimates each. These are designated as trials. The statistical constants of the errors of the ten estimations constituting a trial are the units of calculation in the present investigation. Five of these units constitute a period of the experiment.

The problems to be solved in a study of the influence of

[1] Blakemann, J., *Biometrika*, Vol. 4, pp. 332–350, 1905.

previous experience are two. Is there a modification of the characters of the estimates as a result of experience from period to period? Is there a modification of the characteristics of the estimates as a result of experience from trial to trial within the period?

The answers to these questions have been expressed in terms of the correlation between the number of previous trials within the period of the number of previous periods of experience within the experiment and personal equation or steadiness of judgment as the case may be.

Personal equation seems to be remarkably little influenced by experience. In some experiments it increases, in others it decreases. Taken as a whole the results indicate a slight reduction in personal equation as a result of experience from period to period. Within the period there is no demonstrable influence of experience upon personal equation.

Steadiness of judgment is in rather conspicuous contrast with personal equation, in that it is unmistakably influenced by previous experience. The correlations between the number of previous trials within the period and steadiness of judgment and between the number of previous periods within the experiment and steadiness of judgment are numerically low, but almost without exception indicate that as experience becomes greater the scatter of the individual estimates about their mean value becomes less. Probably the rate of this change is not uniform, but is most rapid at first and then falls off.

There are a number of points concerning these data which might be discussed in greater detail. My purpose, however, has been to present in outline the matters of fact incidentally brought out in the routine of a biologist's experiments. The detailed discussion involved in comparison and interpretation must be left for the psychologist.

THOUGHT-CONTENT AND FEELING

BY KNIGHT DUNLAP

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The criticisms which Professor Lovejoy[1] and Professor Bode[2] have made on my paper on Images and Ideas[3] render it my plain duty to give a more extended exposition of certain points in the theory outlined in that paper. I am forced to conclude that these points have not been fully understood; a situation for which of course my condensed statements are responsible. In the present paper, therefore, I shall attempt exposition rather than argument, hoping that by this means later argumentation may be made more profitable.

At the outset I must insist that my whole point of view is determined by distinctions which the conventional psychologist does not make, and that if the reader fails to note these distinctions he will thereby render it impossible for himself to grasp my point. Furthermore, it must be understood that the terms technically employed in my exposition are always strictly used in accordance with a single definition; a practice which the reader may not expect a psychologist to follow.

The first and most important distinction which I ask the reader to note is between *consciousness* and *content.* By consciousness I mean always the awareness of something, whether the awareness of 'present' content or of 'non-present' content. This consciousness may be called by various other names: 'experience' is one of these, but has other meanings also. *Perception* and *thought* I shall use to designate the two sorts or forms of consciousness: *perceiving*

[1] *The Johns Hopkins Circular*, 1914, 3 (March), 42–99. Professor Lovejoy had the advantage over me in that his article, although appearing in the same number with mine, was written after reading mine and discussing it with me.

[2] *Philosophical Review*, 1915, 24, 102–105.

[3] *The Johns Hopkins Circular*, 1914, 3 (March), 25–41.

and *thinking*, together, to designate all that can be included under *being conscious.*

By *content* (of consciousness) I mean anything which can be perceived or thought of: anything, that is, which can be an object of consciousness.

The value of this distinction may be denied. The psychologists in general do deny it explicitly or implicitly, and use their technical terms in such a way as to cover both of the factors which I insist should be distinguished. 'Sensation' as currently used, for example, means both the object observed (*e. g.*, the color blue, the coldness of the skin) and also the observation of this object.[1] *Consciousness* is also used in this double sense. A writer may on one page speak of the blue as consciousness, and on the next page speak of 'being conscious of the blue.'[2]

The validity of the distinction may also be denied. It is usually the case that an individual to whom I suggest this distinction for the first time is unable to grasp it. This difficulty disappears, however, after he has considered the matter for a little time, so that I see no need of an extended discussion of the question of validity. The question of value can be settled only after full consideration. Having carefully observed a distinction, we may in the end abandon

[1] Frequently the term sensation is used by quasi-psychologists to indicate the nerve process; sometimes the schematic nerve process and sometimes the real process. We need not consider these grosser confusions here.

[2] The term consciousness is also used in a great many other senses, to the great confusion and the lasting reproach of psychology. I have pointed out in another place (Psychological Review, 1912, 19, 407–409) that James, in his *Psychology*, applied the term neither to awareness nor content (except a specific sort of introspected content), but to the subject. What I am here discussing as consciousness, James dismissed as the 'mysterious relation of knowing,' with which psychology, according to him, has no concern. James was concerned, from first to last, with the soul, and his whole psychology was an attempt to devise a scheme by which the soul might plausibly be made an object and examined. Misapprehension of James's intentions has led to the most curious consequences in the hands of American psychologists, who have drawn conceptions and phraseology blindly from James, and the term 'consciousness' has become in their hands so meaningless as to be a byword. It is from this reproach that I should like to see the term rescued.

Note.—Since the above was written, Professor Titchener also has explicitly denied that awareness is any business of psychology (*Amer. J. of Psychol.*, Vol. 26, p. 265). What 'consciousness' is, in Professor Titchener's system, I am not able to make out, in view of the different 'levels' or 'lights' which are attributed to it.

it if it proves fruitless; but to abandon it in the beginning is to beg the question completely.

We must not confuse the distinction between *sentiendum*[1] and *stimulus* with the distinction between *consciousness* and *content*.[2] A stimulus is a form of content whose relation to sentienda must be determined somewhere in our psychology, but which cannot be lightly substituted for sentiendum whenever our handling of the latter gets us in a corner. The content blue of which we were speaking above is the observable blue, an object of vision; whereas the stimulus of blue, the ether vibrations, are not visually perceptible, and it is even a question whether they are perceptible at all.[3]

The most important of the questions raised by Professor Lovejoy concerns the question whether consciousness is or is not an observable fact. If consciousness is not observable, how can we assert that it exists? But can you be conscious of consciousness; be aware of awareness; observe observation? Orthodox psychology says that you can. 'Introspection' as defined by orthodox psychologists (not as used by them when the theoretical discussion is over) has been the term for the process in which awareness is aware of itself. It is necessary to recall the two stock difficulties which are supposed to hamper introspection of this kind. In the first place, a conscious state, when introspected, is not the same as it would be if not introspected. You decide, for example, to observe your consciousness of a caterpillar crawling over a leaf. But when you observe this consciousness, you are conscious not only of the caterpillar and his surroundings, but also of your consciousness of the caterpillar. So the consciousness in the moment for which you planned your

[1] Since the word 'sensation' is so hopelessly slippery in its meanings that it cannot be used without serious misunderstanding, however carefully defined, I have abandoned the attempt to use it and will use 'sentiendum' to designate the simple sense-object, sense datum, or sensibile.

[2] This confusion is not a mere formal possibility. I find in discussing the content-consciousness distinction that it is really difficult in certain cases to keep my opponent from shifting over to the sentiendum-stimulus distinction.

[3] It may be argued that the stimulus is an object of thought only; not of perception. Certainly it is not a sentiendum, but is strictly an intellectual content; in other words, it is relational, not sensory.

observation is far different from the consciousness you planned to observe. For simplicity's sake, you may stop here and conclude that the observation of a mental state changes the state itself. For sanity's sake, also, you should stop here, because if you go on to consider that since the consciousness you are conscious of includes the consciousness of that consciousness, it is also the consciousness of the consciousness of the consciousness, and that therefore it is the consciousness of—but that way madness lies. So much for the first great objection to introspection.

To avoid the first objection, psychologists have usually been willing to face a second which they consider less vital. The state of consciousness is never observed at the moment in which it occurs, but is observed retrospectively, *i. e.*, in memory. In a given moment, according to the orthodox psychologist, you are conscious of the caterpillar. This consciousness occurs as a given fact or process, and cannot afterwards be changed or modified any more than any other historical fact; any more, for example, than your falling over a chair can be changed after you have done it. In the next moments you are able to examine *in memory* the former state just as you are able, as you lie on the floor, to recollect how you fell. This theory of introspection courts the objections, first, that since introspection depends on memory it cannot be accurate, and second, and still more embarrassing, that if the conscious state cannot be directly observed, it cannot be remembered, since it appears in general that there is 'nothing in thought which was not first in perception.' The first objection, psychologists meet by pointing out that the error in observation may be small if the time interval be made small; the second objection they unanimously ignore.

I insist that no introspection of this sort is possible. Neither in the moment of its occurrence, nor in any other moment, is the awareness of an awareness possible. And I defy anyone to find anywhere in the literature of so-called experimental psychology an account of anything which can seriously be called such an observation. What is really observed, as set forth in such reports, I shall consider shortly.

Further than this, I claim that neither in introspection nor in any other kind of observation do we find any kind of 'mental' objects, 'psychic' objects, or any content other than such things as are usually called physical objects. So that introspection as the examination of some form of content different from real things, and by nature the private property of the individual, is excluded, along with introspection as the examination of consciousness itself.

Now comes Professor Bode, and accuses me of not being sufficiently and consistently radical. He states that although I take exception to the evidence of introspection in behalf of images, my objection is not based on the ground that this evidence involves a highly questionable assumption, but rather on the ground that *introspection* reveals muscle 'sensation' rather than images. The question I have raised concerning images is, therefore, according to Professor Bode, entirely artificial because it assumes the existence of a psychical existence which conceals its true nature from everyone except an occasional introspectionist. The 'muscle sensation,' of which (unfortunately) I have spoken, is alleged to be just as undemonstrable as the 'image' of the orthodox psychologist. Professor Bode continues the objection by assigning a reason why I make this questionable and damaging assumption. According to him, Professor Lovejoy and myself both ruin our arguments at the outset by postulating a transcendent power of thought. This postulation, it is true, I do make; but as this assumption is not really the source of my hypothesis of muscle-process as thought-content, although it coheres strongly with it; and as I am unable to make out what Bode's objection to the postulate is, I shall not consider it, except indirectly, in the present paper.

The main objection which Professor Bode raises merits careful consideration, for it is the most serious obstacle in the way of explaining my hypothesis to those who hold tenaciously to conservative views. This objection is in the present case founded on my unfortunate use of the word 'sensation'; and on the ambiguity of the concept of 'introspection' which permits the persistent dualist to take in a dualistic sense my

statement meant in quite a different sense; and this whether I use the term 'introspection' or not. If a dualistic interpretation be put on certain of my statements, they of course become inconsistent with my expressed purpose of attacking epistemological dualism.

The charge that I depend too much on introspection, coming from a philosopher of Bode's training is a surprise, but a pleasant one. As a matter of fact, I had been prepared for exactly the opposite charge: that I have based my hypothesis, not enough on direct observation of the facts involved, but too much on physiology. Not that I counted much of my argumentative material as really physiology, but it is the sort of stuff that many persons suppose to be physiological. I am inclined to agree with Bode that my appeal is really in large measure to introspection: not to introspection as it is usually defined, but to introspection as it really is. It is at this point therefore that I may well begin my explication.

Scientific procedure is full of assumptions, explicit and implicit; and the most useful operation is to make the implicit ones explicit. Bode implies an assumption which when made explicit seems clearly infertile. He implies that in dealing with certain contents or objects it is not useful to make a direct examination of the contents themselves. On the contrary, it seems to me a necessary assumption that the scientific study of any contents or objects must start with the observation of the objects themselves, although it must not stop there. A study of Heppelwhite furniture, for example, which avoids examining any of the pieces themselves, is obviously abortive. So any consideration of the vital question as to the reality of a certain hypothetical form of thought-content which does not involve some observation of actual thought-content must necessarily prove futile. The more difficult the observation the less dependence can we place on the result of any single observation, and the more needful is the study of the conditions under which the content seems to arise: but no amount of difficulty in observing renders it permissible to omit the observations.

Let us consider now what can be observed, either in the observation which we call perception or in the observation which we call thought. The observable things in the universe may be classified in various ways, but the most useful procedure is to reduce these facts by analysis to simple or elementary data and classify these. Or at least we may reduce to facts which, if not *elements* in the logical sense, are *radicals*. The classification of the entire system of radical data in four groups is usually considered exhaustive, the four groups being: (1) sentienda or sense-data; (2) relations; (3) images; and (4) feelings or affects. Few psychologists, so far as I know, have claimed the ability to observe anything else. These 'few' include those who, like Professor Calkins, seem to claim that they have observed their own Egos, and possibly should include certain adherents of the imageless thought school who seem to have observed something in German which as yet has not been observed in English.

The objection most generally raised to the classification is that it is unduly extended. Some, like Ziehen, insist that image and sense-datum are in the same class, and others, like Stumpf, seem to classify feeling and sense-datum together. The difficulty in evaluation of these viewpoints is due in part to the fluidity of terms we have above considered and is enhanced by the divergences in range of fluidity between the German terms and their nearest English equivalents.

Restricting ourselves to content for our observable facts our first business is to observe them. When the question arises whether a feeling is merely a sense-datum, we observe specimens of the two classes and compare them. And we observe also complexes in which the two occur, and compare these. We do not merely, for example, compare pleasure with red, we compare a chair with sorrow, and ask if the latter belongs in the same class with the former. These comparisons as usually carried out lead to no consensus of opinion. Some observers report yes, some report no. In a similar way the chair I think of is to be compared with the chair I see. As to the relation between these there is again a difference of

opinion which is large but not necessarily discouraging. Unaided observation, or rather, observation which is not properly complicated, gives in such cases as these some of the truth but not all of the truth, and possibly more than the truth. Some interesting points of agreement come out however: with regard to such a content as hunger, for instance. Some persons localize it: it is as surely, if not as definitely, in the thoracic region of the body as the color of a rose is on the petals. Such persons, from what I am able to ascertain, consider hunger to be as truly a sense-datum as is color or sound. Other persons fail entirely to localize hunger. It is for these persons as general and unspatial as pleasure or interest, and correspondingly these persons class it with the feelings.[1]

Since simple observation brings us to no definite conclusion we must utilize aids. That is to say, instead of confining our investigation to the objects of prime importance we begin to observe objects and processes which may be connected with these other objects in some special way. We turn most usefully to the consideration of the nervous system. We find that the perception of a simple sense-datum depends on the stimulation of an afferent nerve ending.[2] Color is perceived when the rods or cones in the retina are stimulated in a certain way. Flavors are perceived when certain cells in the taste buds are stimulated, and so on. The evidence, such as it is, points to the conclusion that peripheral stimulation is the *sine qua non* of sense perception. If this is the case with sense perception, it is entirely possible that the perception of feeling may result from the stimulation of the nerve endings in the muscles (especially the smooth muscle

[1] This result raises an interesting hypothesis, namely, that the observable characteristic which distinguishes a feeling from a sense-datum is nothing but spatial localization.

[2] Notice that we say the *perception* of a sense-datum depends on nervous action. It is customary, by using the same term (sensation) for both the sense-datum and the perception thereof to pass from the known fact that the occurrence of the *perception* is physiologically conditioned, over to the assumption that the existence of the *datum* is physiologically conditioned. This usage is one of the most fertile of the centers of confusion in psychology and being almost universally adopted has played an important part in the prevention of psychological progress.

which encircles the blood vessels, the alimentary canal, and the ducts of the larger glands) and the endings in the mucous and serous membranes and in the connective tissues: or from the stimulation of certain of these endings.

This is no new point of view. It has been prominent since 1884 when James (then assistant professor of philosophy at Harvard) published his essay on the emotions, although James's theory did not consider the feelings at all but considered merely non-affective factors in the emotions. Lange (professor of pathological anatomy in the University of Copenhagen) published the following year a theory somewhat agreeing with that of James. And it has since become customary to dub either of these theories, or theories resembling them, the "James-Lange theory." Lange and James derived their theories from Darwin's "Expression of the Emotions." Darwin did not arrive at either theory, but his account of the so-called expression of the emotions as a survival of instinctive behavior which was originally useful, would suggest the "James-Lange theory" to any acute physiologist interested in the causes of emotion. As to the general primary condition of an emotion, James is quite clear; it is activity of the bodily tissues—chiefly the viscera—stimulating the end organs contained therein. But as to the fundamental nature of the emotion itself, James is not so clear. Apparently the emotion is the 'state of consciousness' aroused by the process beginning with these visceral endings. But it is difficult to make this out with certainty in view of James's peculiar use of the term 'consciousness.'

Lange is clearer. The emotion *is* the bodily activity or condition. And this is the view the realist must inevitably take. The analogy is perfect between the sensible object which stimulates the retina, or the olfactory membrane; and the muscle contraction, or other tissue change which stimulates the nerve endings in, or adjacent to, the tissue. Moreover, the emotion, since it is an observable object, must be a real thing, or an aspect of a real thing, and not some quasi-object in a realm of 'psychic' pseudo-reality.

Now an emotion, according to the customary interpre-

tation of the 'James-Lange theory,' is not wholly organic sensation (I use the word 'sensation' here because it is customarily used in this connection), but includes certain elements of feeling, such as pleasure and pain. And it is the nature of these elements which we are investigating. James, however, does not reduce the feelings to organic 'sensations.' In fact he has little to say about the feelings, but makes it rather clear that they are not organic 'sensation.' He speaks of them as of *cerebral origin*, meaning that they are primary feelings of the special senses; they are strictly optical, auricular, etc.

When I say that this theory of feelings as organic processes dates from James's articles, I mean merely that it is the obvious development of James's theory, although neither James nor anyone else (except possibly Lange), so far as I know, ever identified feelings with organic processes. Stumpf it is true, insists that feelings are 'sensations'—"accessory sensations"—but apparently this is the same concept which James had with regard to feeling, and has no intelligible reference to an organic or visceral basis.

We must now, I think, go the whole length. Feelings, as well as the non-affective components in emotion, are bodily states or bodily changes. Emotion is not to be considered as made up of bodily 'sensation' *plus* feeling, but of bodily processes *including* feeling. We must follow Lange, rather than James, in making the emotion not a *result* of the bodily state, but the *bodily state itself.* It is true, as James says, that we do not tremble *because* we are afraid: but it is not true that we are afraid because we tremble. The trembling *is* the fear.[1]

The process of substituting a solid reality for the ghostly 'psychic object' begins, then, in the realm of feeling. It might just as well have begun in the realm of thought-content,

[1] Of course, here we are qualifying, as James does. The trembling is not the major feature of the fear; but it is selected to represent the complex because it is easily observed. The visceral changes—in heart, blood vessels, intestines and glands—are the significant parts of this complex emotion: the trembling of the limbs is mentioned as representing the emotions merely because it is the most *readily visible* feature, and we have a tendency, in observing, to emphasize visual observation; as we shall see later.

and wherever it begins it must spread, in the name of consistency and completeness, over both feeling and imagery. We have no more solid reason for continuing the romantic fiction of a world of 'mental images' than we have for supposing a world of purely 'mental' feelings. Nor would this hypothesis have arisen except as a result of a strongly dualistic philosophy. To the poetic genius of Malebranche, and the forensic genius of Locke (who was steeped in Malebranche's theories), we owe the blighting dualism in psychology.

When we simmer down the mass of so-called introspective evidence for 'images,' we find in the midst of the contradictions, this agreement; that in addition to the *absent* content or object, there is a *present* content. When I think of the gilded dome of the Congressional Library, there is involved something more than the dome and my consciousness of it. This something more is what is misrepresented as the '*image*' of the dome.

I shall not dwell on the details which make us doubt the accuracy of the observations which give us account of the exact features of these 'mental images.' I shall merely emphasize the fact that these observations, in spite of their obvious failings, point strongly to the conclusion that something has been observed: that there is something of the nature of present content before consciousness, at the moment of occurrence of thought, and functionally connected with the thought.

What can this content be? Several factors lead us in the same direction. The important parts that so-called 'muscle sensation' and 'muscle image' play in thought processes (ideomotor processes: motor imagery): the presumption that the neural condition of thought-consciousness (like that of sense-perception and that of affective consciousness) begins with a peripheral stimulus: the facts of perception-building and the association of ideas: all point directly and clearly to muscular contractions as the secondary thought-content. And the muscle contractions seem to fit all the requirements of the case.

The differences between my present statements and my earlier attempts to explain my theory of imagination seem worth emphasizing. First, I have abandoned the term 'muscular sensation' or 'kinaesthetic sensation' because it has been persistently misunderstood; and second, I do not insist upon a sharp distinction between immediate (or secondary) thought-contents and feelings. Feeling may involve the action of the voluntary muscle, although its most characteristic factors are probably due to smooth muscle. Thought-content may involve smooth muscle, in cases where 'affective association' occurs, although the more highly specialized thought-content is activity of striated muscle. I shall not now go over the arguments for the muscle-process. That it furnishes a basis (and the only basis which has yet been suggested) for both the building up of perception and for the so-called 'association of ideas,' I believe I have shown intelligibly in my article in the *Circular*. It remains but to make clear my view as to the aspects of the muscle-process presented to consciousness, and in so doing to remove certain specious objections to the hypothesis of flesh and blood feelings as well as to the hypothesis of fleshy 'images.'

If we examine a muscle and its contraction, we find that the contraction may be observed in several ways. First I may *see* it: if my forearm is bared and I clench my fist, I may observe the swelling, and even the shortening, of the muscles of the arm. If I should have the skin and fascia removed from the arm, the muscle action could be observed in still more detail. (For such observations, however, it is customary and more convenient to use the hind leg of a frog.) Now, if I put my finger tips on the muscle, I may perceive through *touch* the same contraction; that is, the perceptual process is initiated through the nerve terminals of the finger tips instead of the nerve terminals of the retina.

The muscular contraction cannot be heard, and I doubt if it could be tasted, although the sarcolactic acid, the production of which is an essential part of the contraction, is gustible. But there is a set of nerve terminals within the muscle itself through which we perceive the muscle contrac-

tion, and this mode of perception we may call *myoesthesis*, as we called the former two *vision* and *touch*. Now when we analyze the sensible muscle into its component sense-data, we recognize the visual, the tactual and the myoesthetic sentienda. And when we go a step farther and call them 'sensations' we are apt to forget that they are really component parts of the actual muscle contraction, and suppose we are dealing with some unearthly 'psychic' elements which have only a conjectural connection with the real muscle. (I use conjectural advisedly, in view of the theories of interaction and parallelism.) Hence we must avoid the term 'sensation' and cling to the fact that, whether through the intermediacy of touch or through myoesthesis, and whether with accuracy or with abominable error, we are truly observing the muscle contraction.

Now we may clear up two points which have not been clear to Professor Bode and others. Observation through myoesthesis, and observation through other somatic and visceral senses may legitimately be called *introspection*, as opposed to *external observations* through vision, touch, etc. The results of this sort of observations, as I have said before, are what we usually find given in accounts of investigation by the 'introspective method.'

Introspection, as I use the term, is no more esoteric or mystical than external observations. That accurate observation through the nerves of the skeletal muscles and of the viscera is more difficult than observations through the optic and auditory nerves, merely makes us weigh the evidence of the former more carefully. No one can legitimately object to introspection of the sort here explained any more than to any other form of observation: unless he would insist on scientific hypothesis being absolutely divorced from observation!

So much for the confusion over introspection. Professor Bode's second point of difficulty concerns the possibility of a feeling or 'image' occurring without being attended to, or without even consciousness of it. This possibility (which is an actuality in a large proportion of cases) is disturbing to

Professor Bode merely because he persists in interpreting the myoesthetic content as a part of consciousness itself. Since the so-called 'muscle-sensation' is no more than, but as much as, a feature of the muscle contraction, its existence does not necessarily involve consciousness thereof. No one can reasonably doubt that contraction of the biceps can be provoked in a patient completely anæsthetized. Why, therefore, may not a similar contraction occur in the case of an individual conscious of other things yet not conscious of that contraction? Certainly there is no proof against it, and such an occurrence is really quite intelligible. Not every afferent current produces consciousness, and not every object is an object of consciousness. It seems to me a reasonable supposition that the clock out in the hall is still existing and is still ticking away although nobody hears it; and an equally reasonable supposition that muscular contraction may occur and not be perceived.

The actual conditions of consciousness are far from being known. I have sketched in another place an hypothesis of the neural circumstances under which an arc-reflex may in one case condition consciousness, and in another case not. This need not be dragged in here, for the general fact is clear that the determination of consciousness by a given stimulus is variable.

It is therefore no mystical doctrine I am propounding when I say that the present content of thought may be overlooked in attending to the ultimate content. I am not, as Professor Bode seems to suppose, falling back on a subconscious mental process, but am outlining a scheme on which this objectionable subconsciousness may be explained away.

To some persons I may seem to be reducing everything to physiology, destroying any possibility of a psychology, as well as dragging in the mire some of the things (emotions) which we are accustomed to value most highly; but it seems assured that it is not on this ground that I shall be most vigorously attacked, but on the ground that my scheme is idealistic or mystical. For the principle of real content for all consciousness involves a real consciousness in which the

ego is cognizant of these contents, and the conclusion that the present content in thought is muscular contraction, does not allow us to stop here, but leads to the further conclusion that thought is a transcendent function which reaches out across the limits of time and space, and even of existence, to grasp its primary object.

There is an objection of Professor Lovejoy's to muscle-process as the present-content in thought which is similar to Professor Bode's objection, but which is not based on a misunderstanding of what I have meant by 'muscle sensation.' I recognize, according to Professor Lovejoy, a class of content accessible to introspection only, 'muscle sensation,' and although I escape dualism I fall heir to all of the evils I allege to be inherent in dualism. "It would thus follow that the epistemological monist may take, with regard to introspection, much the same position as his dualistic opponent; he too may acknowledge the existence of purely introspectible material, in a natural and definite sense of the expression—which for all practical purposes of psychology is the same sense as that in which the term is employed by the representationalist" (p. 67). In the continuation of this argument, Professor Lovejoy insists that my 'muscle sensations' are not spatial and are in all ways as 'subjective' as the images I reject.

The doctrine of a private psychic content has arisen, it is true, because there is an aspect of muscle contraction, and an aspect of organic process generally, which is observable by only one observer. But the difference between the dualistic hypothesis and my hypothesis is world wide. Dualism insists that there are two classes of objective reality: *perceptible content*, which is in the mind of the perceiver, and which, by its nature, is perceptible only to him; and a substratum of *matter* which, if perceptible at all, is perceptible to several persons. Strictly, there is no shared content on a dualistic scheme; our universes are strictly private; I have my private world and you have yours—if you exist at all. Matter is common property in the sense that we share alike the inability to observe it. In strict dualism, there is no possi-

bility of inferring from my content to your content or to you, because there is not a single experienced factor common to both. There is however a 'soft' dualism which, without saying much about it, slips into the scheme a few things which may be perceived by several persons. As the 'soft' dualism is on its road to realism, I cannot consider it necessary to argue with it.

In a world of real things, which can be experienced, we can infer from certain facts now before us, to certain others not before us, but which are associated in past experience with the facts. For example, I can infer that there is a meat in this walnut, though neither I nor anyone else has seen, felt, tasted or smelled it. Is the meat any less physical, any less real, any more 'subjective' as the term is used, because no one has seen it?

Let us take an example closer to the facts we are discussing. Is the calf of John Smith's leg, which is seen only by John Smith, more subjective, in the philosophical sense, than the calf of an athlete who exposes it to the gaze of ten thousand persons? Does one leg differ from the other in order of reality? Is one physical, the other merely psychic? Now why is John Smith's calf private content, and the athlete's leg not? Is not the reason found in the fact that the one is not allowed to stimulate the eye of any one except Smith? If Smith should cut his trousers off at the knee would his calf not cease to be private content?

The comparison between the two objects (calves) as seen, is exactly like that between the athlete's muscles as seen and as perceived through myoesthesis. The contraction of the muscle can stimulate many eyes; it can, because of anatomical limitations, stimulate only one set of muscle-spindles. But if some of John Smith's nerve endings, from his own calf, were transplanted to a muscle-spindle in the athlete's calf, leaving the nerve connections intact, Smith might myoesthetically experience the athlete's muscular contraction.

Knowing therefore that the myoesthetic aspect of muscle is a real fact, of the same order as the aspects I perceive through other senses, I may infer from the fact that my muscle

has this aspect to the fact that other muscles, having all other aspects, have this one also.

In such inferences, I may make mistakes: some nuts, be it remembered, are withered, or have worms inside; but if I made an adequately minute and detailed study of the outsides and insides of many nuts, I should therefore not be mistaken as to the insides of nuts whose external character I carefully observe. I may be mistaken in ascribing feelings and myoesthetic content to certain living bodies; but it is my observation that is at fault, and not any inherent impossibility which prevents the inference from truth.

Now, it is just this argument from the given content to the content which is not given which it seems to me Professor Lovejoy abuses in comparing it with the argument from the idea to the object.

If you perceived *a*, *b*, and *c* conjoined with *d* in one case, you may infer in another case where *a*, *b* and *c* are behaving in the same way as in the first case, that *d* is there also conjoined with them. This is far different from inferring a connection or conjunction which never has been directly perceived to exist, and in which one of the terms is, by hypothesis, imperceptible.

As regards the spatial character of the muscular contraction, it may be pointed out that the kinesthetic aspect is just as spatial as the visible aspect. The muscle-aspect which I perceive kinesthetically is just as truly extensive, and is just as definitely located, as the muscle which I see. It is true that it is not the sole occupant of a given space, but neither is anything else a sole occupant of its space. Is it not the essential condition of a real thing, such as a muscle, that is made up of several elements located cospatially? Do not the color and the smoothness of this table top occupy the same surface? And does not the color of every part of the interior coexist in the same space with other attributes or aspects? The questions of extensity and location must always be decided by appeal to the direct data of observation: in the case of the muscles as myoesthetically perceived, therefore, by appeal to introspection. I can see

no other way of deciding between the older theory, that the myoesthetic object is the only fundamentally spatial content; Professor Lovejoy's suggestion, that it is not at all spatial; and my own conclusion, that it, and other seeming contents, are equally spatial, except by observing the same data carefully with the questions in view. I feel confident that everyone who makes the observation without confusion of stimulus and sentiendum will find the same result.

The most impressive objections to psychological realism are drawn from the consideration of illusion and error. While the points raised by Professor Lovejoy are not strictly relevant to the purpose of the present paper, I may appropriately indicate certain facts which are important in dealing with these matters. I should not claim that observation always gives us the object perfectly, and I do not see that the explanation of this inadequacy is any more difficult for the realist than for the dualist. The dualist's usual explanation, in fact, is nothing more than a restatement of the problem. Dreams, as well as the 'false' imaginings, may be explained as confused thought-consciousness of real objects which were previously perceived, as well as by calling them the perception of unique private contents.[1]

The strongest impulse towards the dualistic statement of error comes, I think, from a common but groundless assumption concerning color vision, which is directly connected with the theory of the 'relativity of sensation' (meaning sense-data). I deny emphatically that there is any such relativity as is usually described. Red is red and blue is blue, and never the twain shall meet! Intensity, extensity, relational setting (including space) may all be relative, but quality certainly is not. This is an important matter: for the structure of dualism is built on the theory of qualitative relativity, chiefly the alleged relativity of visual sense-data. The new realists are hard put to explain how two skeins of worsted

[1] As for the various spatial illusions, I have no present theory which explains them. Is the stick in the water bent, or is it straight as it was before putting it in? Or is it both straight and bent? Is the edge of the razor smooth, as it appears to the unaided eye, or is it jagged as it appears under the microscope, or both? Interesting logical questions, and not necessarily insoluble.

may appear of the same color to one man, and of an entirely different color to another. Possibly the true explanation is far less complicated than some that the new realists have constructed. We may reasonably assume that the colors in the case cited are different in total complexity, but that they contain identical components, which alone are perceived by the color-blind man.

As common sense realists we are certain that other human beings and other animals have feelings, and other organic processes which might be called *images* (if we should disregard etymology). Means are now at hand for determining whether other persons perceive these feelings, and experimental means for making similar determinations on animals may be devised in the future. A social psychology is therefore possible, and a partial animal psychology is possible. A complete animal psychology depends upon the discovering of critical methods. I must confess that this escape from solipsism, an escape managed without illicitly scaling the fences which a dualist builds for himself, is one of the most satisfying results of realism: the contemplation of the external world as real, and as experiencible by many others, puts my *soma* and *viscera* into the conditions properly described as comfort, happiness and satisfaction.

The probability of a dignified and useful compromise between psychology and the theory of subconsciousness is also gratifying. The popular concept of the subconscious is obnoxious to orthodox psychologists because it is applied most often to consciousness; and the existence of consciousness whose prime and only certain reality is reduced to the zero point has always seemed contradictory. But when applied to *content* of thought, the concept is entirely unobjectionable, and its discussion may be valuable.

Let us suppose, for example, a train of ideas which starts from a given point and leads to a certain other point. Normally, the person is very little, if any, conscious of the series of muscular contractions which are essential to the train of ideas. But even if there were consciousness neither of the contractions, nor of the 'absent' or primary content associated

with or corresponding with them, the series of contractions might go on in the same order. Now if the final reflex in the given series does arouse consciousness, it will be of the same thought-content which would be thought of if the complete series had been fully conscious. For example: looking at the tower of McCoy Hall suggests the tower on Gilman Hall; this brings in the thought of the clangorous chimes; next I think of the chimes at Mills College; then of Mrs. Mills, the founder of Mills College; and next the former president of the board of trustees, who is now occupying a chair at Yale. Yale suggests Professor Angier of the psychological department. Now each of these ideas depends in part on a muscular contraction which connects one arc-reflex with another; and if this series of reflexes, or a slightly different series of reflexes, connected by the same contractions, should run off while I am contemplating the McCoy Hall tower, I might find myself, at the moment of turning away from the window, thinking of Professor Angier without having any recollection of intermediate thought-content, for the simple reason that the only contents really involved in the case (the muscular contractions), while they really existed, were potential but not actual contents: they existed subconsciously. Possibly we can account, in these terms, for most of the reported cases of subconscious elaboration without bringing in the mystical hypothesis of 'split off personalities,' or indulging in the meaningless terminology of 'unconscious cerebration.'

Although the problem of interaction is not solved by realism, it is more sharply defined, and the problem as usually formulated, vanishes. The influence of feeling (including will) on bodily action presents no serious difficulty when we realize that the feelings themselves are bodily conditions and processes. The problem of so-called 'ideo-motor action' also is nicely resolved into a phase of the general theory of organic content. Any muscular action can become the vehicle (associate) of any thought. It is natural that the action which brings about a given result should become the associate of the thought of that result, but this is not a neces-

sary condition; almost any action will do. The vigorous warfare which is being carried on against certain theories of ideo-motor action is, I believe, a contest with windmills, and fictitious windmills at that. When the 'mental image,' which has assumed such a rôle in the accounts commonly given of ideo-motor action, is discredited, and the muscular contraction substituted for it, there is no doubt at all as to the place the contraction occupies in the series we call ideo-motor.

I might go on from this point and show how satisfactory is the account of the reduction of voluntary action to automatic action, in terms of the muscle-contraction content; but this is really so obvious, in view of what I have said concerning the subconscious, that I may omit it with economy. Among the other points on which muscle-contraction content has bearing, the most important of all, in my estimation, is the value of the theory as a means of welding together into a satisfactory whole the various parts of psychology, which without such a synthetic theory seems to be a mere heap of junk stolen from the machinery of various sciences. Psychology is indeed a large subject, involving several disciplines which at the present time tend to draw apart. Psychology includes the study of the sentienda (sense-data), of feelings, and of relations; their analysis, comparison, and classification. But this work does not belong to psychology alone, nor is it all of psychology. The study of the behavior of living organisms is also a psychological occupation; but this again is neither the business of psychology alone nor the sole business of psychology. The very fact that the psychological study of behavior is selective; that it is interested in certain phases of behavior; indicates an ulterior guiding principle. In both of these branches of psychology we are interested in those things—contents and processes in contents—which are definitely associated or involved with consciousness. Analytical or objective[1] psychology, and the science of behavior, or praxiology, from a

[1] The use of the term "objective psychology" to indicate the analytic study of the objects of consciousness seems to me more logical than its use as equivalent with psychophysiology or psychobiology.

realistic viewpoint, are the necessary complements of each other, and I do not see how any antagonism between them can be discerned, except such as is read in by dualism. Take away the assumption of an inner world of psychic reality, distinct from the world of perceptible outer objects, and the two disciplines become parts of the same discipline and strive for common goals.

The third business of psychology, the part to which both analytical and behavior psychology contribute, is the study of the mind. At this point the reader may "register surprise" with "business" of detecting an absurd lack of consistency on the part of the author. After kicking the mind out at the door, I welcome it in again at the window. Do not be alarmed: it is not the same mind; it is not the mind which the psychologists have been using as the repository of all perceptible objects, but the mind which they have been striving after in their definitions as the totality of processes of consciousness, which I am willing to admit to the place of honor as the chief subject of the psychologist's study. But here I am exceeding the announced scope of my paper, and so must postpone the consideration of the *mind which can be studied* until the proper occasion arises to argue out the question as to the existence of *consciousness which cannot be studied.*

PHOTOMETRIC CONSIDERATIONS PERTAINING TO VISUAL STIMULI

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SYNOPSIS

Light and energy not identical but behave in a parallel way.

I. PHOTOMETRIC CONCEPTIONS

1. *The Point Source.*—Luminous flux. Intensity of source. Illumination. The inverse square law. Standards of intensity. Deviations from the ideal point source.

2. *Extended Sources.*—Normal brightness as distinguished from intensity of an indefinitely small plane source. Normal brightness and luminous intensity at an angle with the normal. An extended source of finite area and its deviation from the inverse square law.

3. *Reflection*—regular, diffuse and mixed. Coefficient of reflection.

4. *Transmission*—also of three types analogous to those of reflection.

II. VISUAL OBJECTS

Reflectors and transmitters as secondary light-sources and as visual objects.

1. *Brightness* of a perfectly diffusing reflector or transmitter or of a perfectly diffusing radiator is independent of its extent, distance or of the angle from which it is observed. Brightness varies with the angle in the cases of mixed reflection and transmission. Brightness of a perfect diffuser in terms of illumination. Two mathematical ways of arriving at brightness.

2. *Images* as visual objects. Refracted and reflected images. The plane mirror and specular reflection.

3. *The measurement of brightness.*

III. THE RELATION OF LIGHT TO ENERGY

Luminous flux is conditioned by the rate of flow of radiant energy (radiant power). Mean and specific stimulus coefficients. Their variability. The several criteria for equality of lights of different spectra.

INTRODUCTORY

The purpose of the present paper is to put in brief form a statement of some of the fundamental physical conceptions which apply to visual stimuli. There seems to be confusion of thought in regard to this subject on the part of investigators in those branches of biological science which deal with light-effects, evident in the literature and in discussion.

The subject is treated from the point of view of one who is a student of physiological optics and has had the benefit of association with others familiar with the principles and practice of photometry and with the physics of radiation. There is no attempt made to give technical methods further than these can be used in the discussion as concrete examples to make the points clear.

Definitions, terminology and symbols are, as far as possible, made to conform with the Report of the Committee on Nomenclature and Standards of the Illuminating Engineering Society,[1] which is in fact the starting-point of the work.

An important question arises at once. What is light? There can be no light without radiant energy. Yet radiant energy may be present without light. Light cannot therefore be identified with radiant energy without endless confusion. Light in the physical sense, or more properly *luminous flux*, is defined as radiant power evaluated according to its capacity to produce the sensation of light. If there is any objection to this definition it is that it is so worded as to state that luminous flux *is* radiant power. It would perhaps be better to state that luminous flux is the stimulus-value of radiant power in producing the sensation of light.

Suffice it to say at present that as long as radiation of identical spectral distribution is considered, luminous flux and radiant power may be treated as behaving in an exactly parallel way. The distinction between the two will be drawn later.

The conception of luminous flux may be made clearer by an illustration. A radiating body emits energy which travels away from it in straight lines. Under constant conditions the amount of energy leaving the body in a unit time is constant. That is, the power leaving the body is constant. If that power has the property of stimulating the retina and producing the sensation of light the resulting luminous flux from the body is also constant. One more point: if we think of the radiating body as within a closed surface, and neglect the absorption of the medium within,

[1] *Trans. I. E. S.*, VII., p. 723 ff., Dec., 1912.

the power intercepted by the surface is the same whatever the shape or size of the surface may be. Likewise the luminous flux intercepted by the surface is independent of the shape and size of the surface.

Photometrists treat of light sources and the luminous flux emanating from them. It is obvious, from physiological considerations, that light must enter the eye to stimulate the retina; and from physical considerations, that every visual object is imaged upon the retina by light, either intrinsically its own as in the case of a flame or lamp filament, or borrowed directly or indirectly from some light source. Hence it is perfectly possible to treat all visual objects as virtual light sources. It is nevertheless convenient to divide the discussion into two parts, in the first of which a few fundamental photometric conceptions will be discussed. In the second, the optical system of the eye will be introduced and the physical significance of visual objects studied.

I. Photometric Considerations

1. *The Point Source.*—Suppose a minute particle of incandescent matter which radiates equally in all directions. This is equivalent to saying that its luminous intensity, I, is equal in all directions. By definition, luminous intensity (*candle power*, expressed in *candles*) is the luminous flux per unit solid angle subtended at the source, and the statement that the particle radiates equally in all directions can therefore mean only that the luminous intensity is equal in all directions.

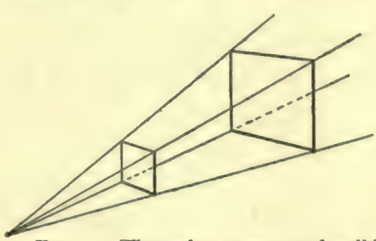

Fig. 1. The point source and solid angle. The light-flux is the same upon any intercepting surface.

It is evident too, that for any small solid angle ω (Fig. 1) the total flux, F, within that angle is the same across any section of it, since radiation travels in straight lines. If this particle is thought of in the center of a sphere of radius r the total flux upon the interior of the sphere will therefore also be the same for any value of r.

Illumination on a surface is defined as the luminous flux-density upon that surface, or the flux per unit of intercepting area.

The flux within the angle ω is $I\omega$. The intercepting area of the spherical surface within that angle is ωr^2. The illumination, E, upon that area is therefore, from the definition, given by the equation:

$$E = \frac{I\omega}{\omega r^2} = \frac{I}{r^2},$$

that is, the illumination is equal to the intensity of the source divided by the square of the distance. This is the well-known inverse square law which is of such great importance to the photometrist.

If instead of a minute portion of the sphere an oblique surface is considered, making an angle θ with the spherical surface, the area of the oblique surface within the solid angle will be larger than the spherical and will be equal to $\omega r^2/\cos\theta$. The illumination upon the oblique surface will therefore be given by the equation:

$$E = \frac{I\omega \cos\theta}{\omega r^2} = \frac{I \cos\theta}{r^2},$$

of which the preceding one is a special case in which $\theta = 0$.

By way of summary a few definitions can now be profitably given, along with the associated terminology.

The standard of luminous intensity is either a flame, produced under certain standard and reproducible conditions, or a particular incandescent electric lamp run upon specified voltage.

The *luminous intensity* of any source is the flux emitted per unit solid angle subtended at the source, and when measured in terms of candles, is its candle-power.

The unit of flux is the *lumen*. It is equal to the flux emitted within a unit solid angle by a point source of one candle, or in other words it is the flux intercepted by a portion of spherical surface whose area is equal to the square of its distance from a point source of one candle situated at its center.

Illumination, on a surface, is the flux per unit of intercepting area. It is of unit value, in the case just described, when the radius of the spherical surface is of unit length and the surface consequently of unit area. The unit may be conveniently remembered as the illumination due to a source of one candle upon a surface at unit distance placed normally to the direction of the flux. The unit of illumination is the *foot-candle* or the *meter-candle* according to the unit of length used. The latter is sometimes called *lux*.

It is to be remembered that no light-source behaves as the hypothetical point source. Standard intensity is obtained from a standard source *only* in a specified direction.

Further, the inverse square law does not practically hold unless certain precautions are taken, as will shortly be seen.

2. *Extended Sources*.—In place of the luminous point just discussed, imagine an infinitely thin and very small incandescent plate. It has a definite area S (Fig. 2) and as a source has also a certain intensity I in a direction toward P, normal to the surface.[1] Other things equal, increase or decrease in the area of the plate will be accompanied by a proportional change in the flux toward P. We can then state that

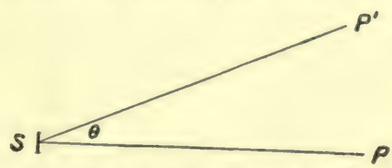

FIG. 2. The element of radiating surface. Its luminous intensity is the product of its *area*, its *brightness* and the *cosine* of the angle between the direction of observation and the normal.

$$I = Sb_0.$$

This introduces a new quantity b_0, the *normal brightness* of the surface S. This is an entirely different quantity from the intensity of S as a source and is on no account to be confused with the intensity. This separation of the two quantities is absolutely imperative where the source is considered as a visual object.

The intensity of such a source in any direction P' other than normal becomes

$$I = Sb_0 \cos \theta.$$

[1] Provided the point P at which I is measured is at a distance very large as compared with the dimensions of S. The reason for this will appear later.

This equation expresses Lambert's law of emission, often called the cosine law, and in general holds true for radiating bodies. It also indicates that practical light sources may have different intensities in various directions.

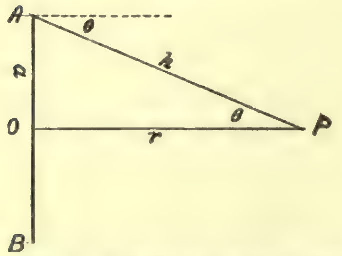

FIG. 3. The extended plane source does not follow the inverse square law.

An extended plane source AB (Fig. 3) examined from a point P, where OP, the normal distance of P, is of the order of magnitude of the diameter of AB, shows a radical departure from the conditions of the ideal point source. The intensity toward P due to a small part dS of the source at O is

$$I = b_0 dS$$

and the illumination upon P due to the same

$$\frac{I}{r^2} = \frac{b_0 dS}{r^2},$$

while the intensity toward P for a similar element of the surface at A is

$$I' = b_0 dS \cos\theta.$$

The illumination at P normal to AP and due to the element at A is then

$$\frac{I'}{h^2} = \frac{b_0 dS \cos\theta}{h^2}.$$

If P is an element of surface parallel to AB it is also oblique to the flux from A, and hence intercepts only a part of it, proportional to $\cos\theta$, and we have then for the illumination on the parallel surface at P due to A

$$\frac{b_0 dS \cos^2\theta}{h^2};$$

putting

$$\cos\theta = \frac{r}{h} \quad \text{and} \quad h^2 = r^2 + a^2$$

we have the illumination on P for an element of AB at A,

$$dE = \frac{b_0 dS r^2}{(r^2 + a^2)^2}$$

and at O,

$$dE = \frac{b_0 dS}{r^2}.$$

From these equations it may be seen that while the illumination at P due to the part O of the surface normal to OP, obeys the inverse square law, the illumination due to other parts (as at A) does not do so except approximately when r is much greater than a.

By integration it is found that if AB be a disc with its center at O, the total illumination at P, normal to OP, is $(Sb_0)/(r^2 + a^2) = (Sb_0)/h^2$, where S is the area of AB. That is, the illumination does not follow the inverse square law but approximates it when r is much greater than a.

3. *Reflection* is of two types, regular and diffuse. In the case of perfect regular reflection, light leaves the surface at an angle equal to that of its incidence on the opposite side of the perpendicular from the point of incidence (Fig. 4).

Fig. 4. Types of reflection

In the case of perfect diffuse reflection, light falling upon the reflecting surface leaves it in all directions with exact similarity to the case of a diffusely radiating body, according to Lambert's cosine law (p. 4). Most objects are of a mixed or intermediate reflecting type, part of the light being reflected regularly, although often diffused or spread to a limited extent, and part being diffusely reflected. Any surface which can be made to give a so-called reflection from a window or light-source, and yet reflects more or less light in all directions

however it is lighted, belongs to this class. Examples are numerous—painted walls, most papers, wood-work of a greater or less degree of polish and so on.

Regular and diffuse reflection are only occasionally approximated. A mirror is a typical example of the former, while white blotting paper, plaster of paris or magnesia surfaces approach perfect diffusion.

Measurement of the reflected lights fails to account for all of the light incident upon the surface. A certain amount of it is lost and is said to be absorbed by the surface. The *coefficient of reflection* is the ratio of the reflected to the incident light-flux. We may distinguish between the two portions of reflected light-flux in cases where typical regular and diffuse reflection coexist, as in the case of a piece of glass backed with white blotting paper, and determine the coefficients of diffuse and regular reflection separately.

Regular Diffuse Mixed

FIG. 5. Types of transmission

4. *Transmitting bodies* offer a parallelism to the different types of reflection (Fig. 5). A clear glass through which objects may be seen transmits regularly, milk glass scatters the light transmitted through it in all directions approximating perfect diffusion, while a ground or frosted glass diffuses part of the light but transmits a large fraction in directions approximating that of incidence.

This statement presupposes the transmitting medium to be a thin parallel-sided layer. Otherwise refraction enters and modifies the result.

As in the case of reflection there is a *coefficient of transmission* which is the ratio of the light-flux transmitted to the incident flux.

With respect to the coefficients of reflection and transmission the parallelism between luminous flux and radiant power breaks down. This is for the reason that all wavelengths of light are not transmitted or reflected in equal proportion and we are therefore dealing with two radiations of different spectral distributions in the radiation before and after incidence respectively. The same reason applies to the case of luminous flux from two sources of different spectral characters, as from a tungsten and a carbon lamp. The source to which it is referable should always be specified along with the coefficient of reflection or transmission, as it is only by way of exception that the coefficient is the same for two sources of different spectral character.

II. Visual Objects

It is obvious that any reflecting or transmitting medium may be considered as a light-source, having different intensities in different directions, depending upon the character of its reflection or transmission and upon the manner of incidence of light upon it. Visual objects are chiefly objects which reflect light, although intrinsically luminous bodies (light-sources) and transmitting media of various kinds are by no means to be excluded from the list. Even images, which have existence only as particular arrangements of light-flux, have to be reckoned as visual objects. In short, any locus in space from which light-flux enters the eye has to be considered as a visual object or a part of one.

A perfect diffuser, considered from this standpoint, follows the cosine law exactly as a radiating surface does, and such bodies are of especial interest as visual objects.

1. *Brightness* is the photometric quantity that determines the lightness or darkness of the object as a visual stimulus. While this is perfectly true, objects of equal brightness do not always look alike. They may give rise to different sensations. A gray surface may, for example, be made to

appear lighter or darker by slipping (respectively) a black or white background behind it, the brightness of the gray having remained unchanged. The same brightness may undergo similar changes of appearance when viewed (respectively) after light has been excluded from the eye for some time, or after exposure of the eye to very bright environment. These changes are due to modifications in the behavior of the visual apparatus and they are peculiarly referable to the eye itself.

In so far as the lightness or darkness of appearance of any object depends upon the object itself and the light conditions under which it is, the quality of its appearance is determined by its *photometric brightness*.

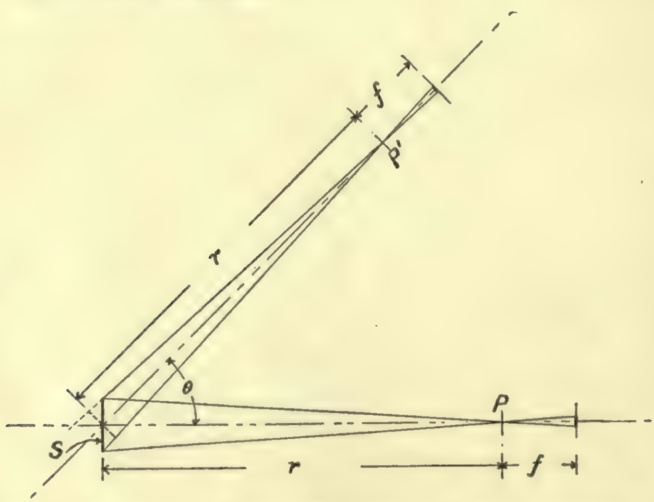

FIG. 6. The photometric brightness of a perfect diffuser determines the illumination of its retinal image independently of the manner of obser vation.

Consider a small surface S (Fig. 6) which may be either radiating, diffusely reflecting or diffusely transmitting. Its intensity in direction P normal to its surface is $I = b_0S$ as shown previously, and in direction P', $I = b_0S \cos \theta$ (pp. 6 and 7).

If viewed by an eye at P, the illumination upon the eye due to S is b_0S/r^2 and the flux through the pupil upon the retina $= (kb_0S)/r^2$ where k is a factor depending upon the pupillary aperture and the absorption by the eye-media.

The area of the retinal image $= (Sf^2)/r^2$ where f is the focal distance of the optical system of the eye. Accordingly the flux-density (illumination) of the retinal image (flux/area)

$$= \frac{kb_0S}{r^2} \cdot \frac{r^2}{Sf^2} = \frac{kb_0}{f^2},$$

which is independent of the distance r and the area of the surface S.

Again, in the direction P' the luminous intensity is $b_0S \cos \theta$ and the flux upon the retina for a perfectly diffusing surface is

$$\frac{kb_0S \cos \theta}{r^2}.$$

The size of the image upon the retina is in this case less than before, owing to so-called foreshortening, and is equal to $(Sf^2 \cos \theta)/r^2$, and the flux-density of the retinal image is

$$\frac{kb_0S \cos \theta}{r^2} \cdot \frac{r^2}{Sf^2 \cos \theta} = \frac{kb_0}{f^2}$$

or exactly the same as when seen from P.

Hence the illumination of the retinal image, that is the light-flux per unit retinal area due to the image, of a perfectly diffusing body is independent of the extent of that body, of the distance from which it is observed, and of the angle made by the line of vision with its surface.

It will be seen by examining the course of reasoning by which this conclusion is reached that it depends upon the fact that the light from a diffusing surface leaves it in such a way that the luminous intensity of any element of the surface in any direction is as the cosine of the angle made by the line of that direction with the normal to that element. It is in fact only exceptional surfaces of which this is true, namely, perfect diffusers, and of them we say that the brightness is equal in all directions. These two facts hold together as just shown, and the uniform brightness for all directions may be directly demonstrated when it is a fact by means of the photometer, which is in its essence no more than an instrument for equating the illumination of two juxtaposed retinal

images. Such a demonstration then proves the cosine law of distribution for the surface examined.

In most cases however we would find by photometric examination that the brightness of the body is different from different angles of observation. This is true of bodies of the mixed reflecting or transmitting type. In such a case we would have shown that the illumination of the retinal image of the surface when examined from the direction P' is not $(kb_0)/f^2$ but something else which, by substituting for b_0 a more general symbol b, we will denote $(kb)/f^2$. By following the preceding mathematical reasoning backward this would give for the illumination upon the eye $(bS \cos \theta)/r^2$, and for the luminous intensity of S in the direction P', $bS \cos \theta$.

By choosing a value of b to satisfy these conditions we have at once a quantity, which multiplied by the projected area of S on a plane normal to the line of observation gives the luminous intensity in that direction; and which is also the photometric quantity referable to the surface itself that directly determines its value as a visual stimulus. It is convenient to note that the brightness b may be arrived at in two ways:

(*a*) *Brightness* in any given direction is defined as the luminous intensity per unit area of the surface projected on a plane perpendicular to the direction considered.[1]

It is only necessary then to know the candle power of the surface, as a source, in the direction considered and divide by the projected area of the surface. The candle power is $bS \cos \theta$, the projected area $S \cos \theta$, and the quotient is the brightness b in candles per unit area.

(*b*) Or the brightness is equal to the normal illumination at a point in the direction considered, due to the flux from the surface in question, divided by the solid angle subtended at that point by the surface. The former is $(bS \cos \theta)/r^2$, the latter $(S \cos \theta)/r^2$, and the quotient b as before.

The brightness of a diffusely reflecting or transmitting body may be computed, knowing the illumination upon it

[1] Provided that the surface is of dimensions negligibly small as compared with the distance to the point of observation. That is, that all parts of the surface are, within negligibly small limits, equidistant from that point.

and its coefficient of reflection or transmission. We have seen that the luminous intensity of an element of diffusing surface is the product of its area, its brightness and the cosine of the angle between the direction of observation and the normal to the surface, that is

$$I = Sb_0 \cos \theta.$$

The light-flux within a small solid angle $d\omega$ in the direction θ is then $Id\omega = Sb_0 \cos \theta d\omega$. The latter part of this expression, $\cos \theta d\omega$, by integration for the entire hemisphere into which the light is emitted becomes π, and the total flux from the surface is therefore πSb_0.

If the illumination upon the surface be E and its reflection-coefficient m the flux emitted must also be equal to EmS and we have the equation: $EmS = \pi Sb_0$, whence $b_0 = Em/\pi$.

The same holds true for a transmitting body if we substitute for m the coefficient of transmission. This presupposes two things: first, we must know that the body is a perfect diffuser, and second, we must know its coefficient of reflection or transmission. In spite of these limitations this formula affords a ready means of approximating the brightness of a surface.[1]

2. *Images* considered as visual objects are interesting as possible experimental stimuli and as affording a convenient means of measuring brightness directly.

The face of a convex spherical lens of say not over 30 cm. focal length, when held at arm's length is seen as filled with

[1] A paper surface with a reflection-coefficient of 80 per cent., having an illumination of 7 meter-candles upon it has a brightness of $(7 \times 0.8)/\pi = 1.78$ candles per square meter, more or less, depending on how closely the conditions approach ideal conditions. Such a rough approximation is of use when, as often happens, the conditions are stated in terms of illumination and we wish to make an intelligent estimate of brightness or *vice versa*.

Since the above was written another unit of brightness has been proposed, the *lambert*. It is defined as the brightness of a perfectly diffusing surface emitting one lumen per square centimeter. On a perfectly diffusing surface of 100 per cent. reflection coefficient the incident light, in lumens per square centimeter, would be numerically equal to its brightness in lamberts. One lumen per square centimeter is 10,000 lumens per square meter or 10,000 meter-candles (lux). One lambert is therefore equal to $10{,}000/\pi$ or 3,183 candles per square meter; or to 0.3183 candles per square centimeter. As a practical unit the *millilambert* is proposed which is 0.001 of this.

an inverted image of distant objects. If of shorter focal length nearer objects may be distinctly seen in the image and a concave lens similarly used presents a reduced erect image. The brightness of an image so observed is easily worked out from the foregoing considerations.

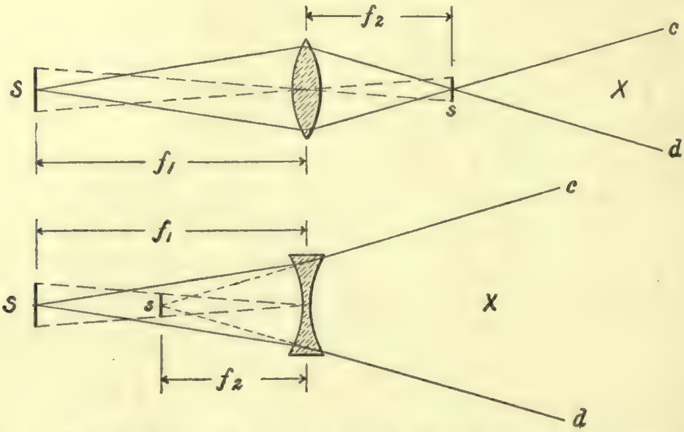

FIG. 7. The brightness of an image directly viewed against a lens-face is equal to the brightness of the object independently of its relative size or manner of observation.

The image s of a small element of the surface S (Fig. 7) sends out light which in effect comes from s to any point lying as X within the angle csd. The flux of the two pencils of light from S and s, limited by the lens aperture, is necessarily equal. That from S passing through the lens aperture whose area is a, is equal to $bS \cdot (a/f_1^2)$ (product of luminous intensity and solid angle).[1] The luminous intensity of s within the limits of the pencil csd is therefore this flux divided by the solid angle csd $(= a/f_2^2)$ and is equal to

$$\frac{bSa}{f_1^2} \cdot \frac{f_2^2}{a} = bS\frac{f_2^2}{f_1^2}.$$

The areas of s and S are as the squares of their respective

[1] The actual pencil of light concerned in vision is not csd but one limited at x by the pupil of the eye. Such a small area of the lens face is used that the error in considering a/f_1^2 and a/f_2^2 to be the respective solid angles is negligible and the relations given hold true.

distances from the lens, therefore,

$$s = S\frac{f_2^2}{f_1^2}.$$

The brightness of s from x is therefore

$$bS\frac{f_2^2}{f_1^2} \div S\frac{f_2^2}{f_1^2} = b.$$

That is, a real or virtual image is just as bright as its object irrespective of their relative sizes or distances.

This of course neglects one factor, the loss of light in passing through the lens. This is constant for a given refractor, a single lens having a transmission coefficient of about 90 per cent.

It will be obvious that the foregoing conclusion applies to the reflected images of the convex and concave mirrors, and to the special case of the plane mirror. The reasoning is exactly parallel. The image is always of a brightness equal to that of the object multiplied by the coefficient of transmission in the case of the refracted image, or by the coefficient of reflection in the case of the reflected image. This applies then to the case of regular reflection, not dealt with in considering the brightness of surfaces. The brightness is in this case referable rather to the image than to the surface.

Regular or 'specular' reflection, as it is sometimes called, often coexists with reflection of the diffuse or what we have here designated as the mixed type. Examples of this are a polished white marble, and a clear glass backed with paper. This of course constitutes another type of mixed reflection. The latter example is mentioned because it shows how the image and the surface-brightness may be concretely added or separated, in exactly the way they would be mathematically considered by the photometrist as two separate brightnesses superposed.

3. *Brightness may be measured* in two ways, either according to its definition, luminous intensity per unit area, or directly by visual comparison with a surface of known brightness.

In the first case suppose the surface AB (Fig. 8) of which o is one point, and a screen at right-angles with op having an opening of area a and placed at a distance r from a photometer p so arranged that from all points of p part of AB is seen completely filling the opening a; and let the distance $op = r$ and the area of AB seen from p be S. The illumination due to AB at p is found by measurement to be E. The luminous intensity of S is then Er^2 which is equal to $bS \cos \theta$ and the projected area is $S \cos \theta$. The brightness is then $Er^2/S \cos \theta$.

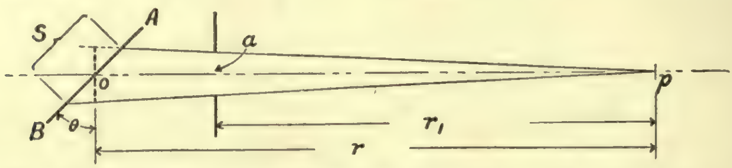

FIG. 8. Brightness measured as luminous intensity per unit projected area.

If however the opening a, normal to op is thought of as the bright surface, we have for the luminous intensity of the opening Er_1^2 and its area a. Its brightness is therefore Er_1^2/a which (since from geometrical considerations $r_1^2/a = r^2/S \cos \theta$) is equal to $Er^2/S \cos \theta$.

Hence one actual way to measure brightness is to place a screen with a small opening between the surface and the point of observation, normal to the line of observation, and determine the brightness of the opening, according to definition. The screen may be done away with if AB is of small dimensions and is in perfectly dark surroundings.

This method is however applicable only to objects of over a certain minimum luminous intensity. The majority of visual objects have a brightness so low that the illumination from them upon the photometer screen under the conditions outlined is too low to make photometric comparison possible. In such cases the method of direct comparison gives satisfactory results.

In this method a portion of the surface S (Fig. 9) to be measured is seen from X in photometric juxtaposition with a surface whose illumination can be controlled and known at least relatively—as for example through an opening in a

screen p illuminated from a lamp L at measurable distance. When the position of the lamp is determined which makes p and S equal, a surface of known brightness can be put in place of S and a similar setting made. By knowing the two distances of the lamp from the screen and the brightness of the standard surface, the brightness of the unknown surface may be computed.

The brightness of an image may be measured in an exactly similar way. The image may however be at S, or perhaps

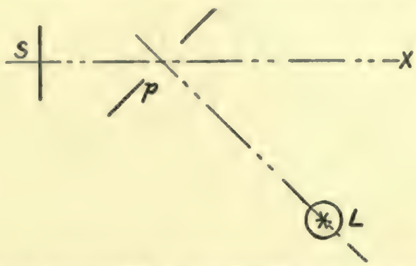

Fig. 9. Brightness measured by comparison with a substituted standard.

more conveniently focused in the opening of p, in either case it must appear from x to fill the opening completely and uniformly.

III. The Relation of Light to Energy

In order that the sensation of light may arise, radiant energy must fall upon the retina. We have seen that brightness, in the photometric sense, is the purely external factor which determines the visual appearance of an object. Photometric measurements are made by making two brightnesses visually equal under conditions equal in all respects, then from the way in which these brightnesses are brought about, computing candle-power, illumination and so on.

Luminous flux is conditioned by the rate of flow of energy. A surface placed normally in the path of light has upon it a certain illumination which multiplied by its area gives the luminous flux that it intercepts. This depends upon the radiant power (energy per unit time) intercepted by it. Luminous flux and radiant power P may be connected by the

simple equation

$$F = K_m P.$$

where K_m is the *mean stimulus coefficient* for the particular kind of radiation used.

For radiation of a particular wave-length,

$$F_\lambda = K_\lambda P_\lambda.$$

K_λ is the stimulus coefficient for the particular wave-length considered, P_λ the radiant power of that wave-length and F_λ the resulting luminous flux.

It is found that K_λ has a variable value for various parts of the spectrum. It rises slowly from zero to a very small value at wave-length 720$\mu\mu$ increasing to a maximum and decreasing again to an insignificant value at perhaps 400$\mu\mu$. Further than this, in any given instance K_λ for identical power conditions varies with different eyes, with the state of the observing eye, and with the brightness at which the photometric comparison is made.

Furthermore, there is still to be settled the point as to the best criterion by which brightnesses may be equated in the case of lights having different spectral distributions, whether by direct comparison, by the so-called flicker method, or by the detail-revealing power of the radiation.

The photometrist, with partial success, avoids this question at the outset, by taking an arbitrary source of radiant energy and assigning to it a certain candle-power, then proceeding, with this as a standard, to measure other sources. In this way he kept out of trouble until he was obliged to compare sources emitting radiation of color (spectral distribution) different from his standard. The fundamentally correct way to make such comparisons is still a matter for discussion and experiment.

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The Psychological Review

THE PLACE OF THE CONDITIONED-REFLEX IN PSYCHOLOGY[1]

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Since the publication two years ago of my somewhat impolite papers against current methods in psychology I have felt it incumbent upon me before making further unpleasant remarks to suggest some method which we might *begin* to use in place of introspection. I have found, as you easily might have predicted, that it is one thing to condemn a long-established method, but quite another thing to suggest anything in its place. I wish in my remarks tonight to report what progress has been made in this direction.

Probably the first question you will insist upon my answering is: "Why try to find a substitute for introspection? It is a pretty good method after all and has served us well." Rather than stop at this tempting place to enter into a controversy, I shall call your attention to the naturalness of such a quest on the part of the students of animal psychology. The truth of the matter is that animal psychologists have become somewhat intoxicated with success. Finding that an amœba will orient more quickly to certain rays of light than to others, and that a blind, anosmic rat can learn to thread its way through a maze, they begin to look at man with a covetous eye: "After all," they argue, "man is an animal; he moves in response to stimuli in his environment, or to the stimuli offered by the displacement of tissue within his own body. Furthermore, he moves in

[1] Address of the President, before the American Psychological Association, Chicago Meeting, December, 1915.

characteristic ways. Why cannot we study his behavior in the same way that we study the behavior of other animals, modifying our methods to suit this new genus?"

We all admit that many problems in the two fields are similar if not identical. This is especially true of sensory problems. All of us alike wish to determine the various groups of stimuli to which our human or infra-human organism will respond; the various amounts of stimulation necessary to produce these responses, and the bodily areas upon which stimuli must impinge in order to be effective.

Now the animal psychologist has met with a certain degree of success in answering such questions. When we contrast animal psychology in 1900 with animal psychology in 1915 we are forced to admire the enormous strides which have been made in defining problems, in evaluating methods, and in refining apparatus. In 1900 we were content to study by crude methods the elementary features of habit formation in a few easily handled vertebrates. 1916 finds us prepared to carry out on animals as low in the scale as the worm far more delicately controlled experiments than were dreamed of in 1900. The present time likewise finds us prepared to undertake upon the higher vertebrates problems in behavior which in 1900 could hardly have been formulated in behavior terminology. In 1900 who thought of comparing visual acuity in different animals by the use of methods as delicate as those we use on the human being? Or who was bold enough then to assert that in a few years' time we should be using methods for studying vision, audition, and habit formation which are more refined than those which have been employed in the study of the human subject? We must admit, I think, that in the infra-human realm, at least, these years of constant effort have given the animal psychologist a right to look with yearning eyes at this proud genus *Homo*, the representatives of which he finds roaming everywhere, eating any kind of food and from almost any hand, and so resistant to climatic changes that only the lightest kind of covering is necessary to keep them in good condition.

Such in part are the motives which have led the animal

behaviorist to push into gatherings to which he has not been especially invited. Whether we should condemn his enterprising spirit or accept him depends upon how he behaves after admittance. If he can justify his position by deeds, I believe he will be accepted, while possibly not to complete fellowship, at least as an individual who will not bring discredit upon his fellow scientists.

The behaviorist, while meeting no theoretical difficulties in his attempts thus to universalize his methods, does, at the very outset of his studies upon man, meet with very practical ones. In sensory problems when we ask such simple questions as, what is the smallest vibration difference between two tones that will serve as a stimulus to reaction in this particular man, or whether sweet and bitter can be reacted to differently by him, we find that there is no objective method ready at hand for answering them. We know how to employ objective methods in answering such questions with animals. But the animal methods are admittedly slow, and, from the standpoint of the human subject, cumbersome. Some years ago I suggested that we ought to begin to use human subjects in our so-called "discrimination boxes." As might have been surmised, no one took my advice. This was due in part at least to man's upright position, his size, and, I might add, his general unwillingness to work under the conditions which must be maintained in animal experimentation. One can scarcely blame the human subject for objecting to being kept for long stretches of time in a home box the door to which opens from time to time permitting him to pass to the right or left of a partition, and ultimately to reach one or the other of two differently colored surfaces below which he finds a food trough. That which makes the situation still more humiliating to him is the fact that if he has "backed" the wrong color he receives a stone in the guise of an electric shock, in place of the bread which he seeks.

I suggested this rather hopeless method of investigating the sensory side of human psychology because of the increasing desire on the part of many psychologists to see psychology begin to break away from the traditions which have held her

bound hand and foot from the establishment of the first psychological laboratory. I believe that the time is here when the most conservative psychologists are willing to give a lenient hearing to even crude experimentation along lines which may possibly yield an objective approach to sensory problems. This belief has emboldened me to describe briefly our work at Hopkins upon the *conditioned reflex.*

Conditioned Reflexes

In discussing the subject of conditioned reflexes it is customary to make a distinction between (*A*) *conditioned secretion reflexes* and (*B*) *conditioned motor reflexes.* Whether there is any genuine distinction between the two types depends, I think, upon what ultimately will be found to be true about the *modus operandi* of the glands (*i. e.*, whether under such conditions muscular activity is essential to glandular activity or whether control of the glands can be attained independently of the muscles through nervous mechanisms).

A. Conditioned Secretion Reflexes

Before taking up the conditioned motor reflex, with which I am most familiar, I wish briefly to call your attention to one of the most widely known conditioned secretion reflexes, viz., the salivary. The conditioned salivary reflex is well known in this country, thanks to the summaries of the researches in Pawlow's laboratory made by Yerkes and Morgulis, and more recently by Morgulis alone. In brief, this method, which has been under experimental control for some eighteen years, depends upon the following fact: If food (or some similar salivating agent) which produces a direct salivary reflex, and, *e. g.*, a flash of light, are offered jointly for a number of times, the light alone will come finally to call out the salivary secretion. To bring this 'reflex' under control it is necessary to fix upon some method for observing the flow of saliva. This is accomplished usually by first making a salivary fistula, and later attaching a glass funnel to the opening of the duct of the gland. The total flow of saliva may then be measured directly or the individual drops

registered graphically. The use of food for arousing the direct flow of saliva has proved to be slow and not very satisfactory. Most of the work has been done by using acid (dilute HCl). The acid produces a salivary flow immediately and with great sureness.

The conditioned salivary reflex has at present no very wide sphere of usefulness or applicability. In the first place it can be used upon but few animals. Up to the present time it has been used largely upon dogs. Even when used upon these animals the method has very serious limitations. The use of acid for any appreciable time produces stomatitis, according to Burmakin. This makes it almost impossible to carry out investigations which extend over long periods of time. Unless some strong saliva-producing agent is used, the reflex quickly disappears and cannot easily be reinforced. In its present form the method (which calls for operative treatment of the subject) can not be used, of course, on man. Dr. Lashley has been making some tests looking towards an extension of the method. He is experimenting with a small disc grooved on one surface, so as to form two concentric but non-communicating chambers (Fig. 1). The outer chamber, by means of a slender tube, communicates with a vacuum

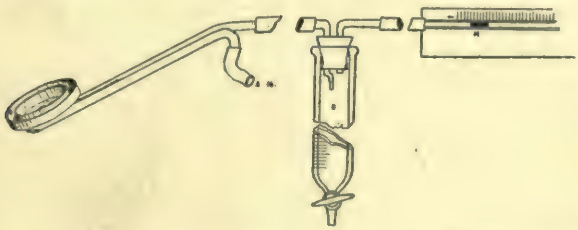


FIG. 1. Apparatus for measuring salivary secretion in man (devised by K. S. Lashley). *o*, outer chamber connecting with vacuum pump, through tube at *AH*. When a vacuum is created the disc clings to the inner surface of the cheek. *i*, inner chamber which is placed over opening of parotid gland. Saliva flows into graduated flask *G* where the total flow of saliva can be measured. Another system of measurement is offered by reason of the fact that when a drop of saliva falls into *G*, air is forced out through a second opening in the flask. When a slender glass tube containing a drop of mercury is attached to this opening, the mercury drop is forced forward a short distance at each drop of saliva. A suitable scale placed behind the glass rod enables one to read and record the number of drops of saliva which fall during any part of the total reaction. (*Sialometer* is an appropriate name for this instrument.)

pump. When the air is exhausted the disc will cling to the inner surface of the cheek. The inner chamber, which is placed directly over the opening of the salivary gland, is likewise supplied with a slender tube which passes out through the mouth. The saliva passing out through this tube can be recorded in different ways. It is too early to make any predictions concerning the usefulness of such a method.

Of the possibility of extending investigation to other forms of secretion, little at present can be said. The work of Cannon, Carleson, Crile, and others, has opened our eyes to the extent to which glandular and muscular activity are called into play in the simplest forms of emotional response. The human psychologist has too long subordinated everything to the obtaining of a vocal response from the subject, while the animal psychologist has too long subordinated all to the obtaining of response in the muscles used in locomotion. Both have failed to work out methods for observing the finer changes that go on in that large class of actions that we call *emotional*. Until recent years we have been lacking proper indicators of such changes. I believe that the conditioned secretory reflex, in one form or another, can be made useful in these fields.

While recognizing the importance to all psychological students of Pawlow's work on secretion reflexes, our own work has centered around the conditioned motor reflex of Bechterew, since we find for his method an immediate and widespread usefulness.

B. The Conditioned Motor Reflex (*Bechterew*)

The *conditioned motor reflex*, while familiar in a general way to everyone, has not, so far as I know, engaged the attention of American investigators. This is not surprising in view of the fact that all of the researches have appeared in Russian and in periodicals which are not accessible at present to American students. At least we have not been able to obtain access to a single research publication. The German and French translations of Bechterew's 'Objective Psychology' give the method only in the barest outline. Bechterew's summary was the only guide we had in our work at Hopkins.

We may give a few examples from daily life of conditioned *motor* reflexes. In the moving picture tragedies the suicide of the villain is often shown. Usually the hand only of this unfortunate individual is displayed grasping a revolver which points towards the place where his head ought to be. The sight of the movement of the hammer on the revolver brings out in many spectators the same defensive bodily reaction that the noise of the explosion would call out. Again we find in persons recently operated upon numerous reactions such as deep inspirations, cries of pain, pronounced muscular movements, the stimuli to which are the cut and torn tissues themselves. For many days after the disappearance of the noxious stimuli the reactions will appear at the slightest turn of the subject's body or even at a threat of touching the wound. Similar instances of this can be seen in many chronic cases. In such cases the charitable physician characterizes the patient as having "too great a sensitivity to pain." The patient, however, is not shamming in the ordinary sense: conditioned reflexes have been set up and the subject makes the same profound reactions to ordinary attendant stimuli that he would make to the noxious stimuli themselves.[1]

For almost a year Dr. Lashley and I have been at work upon the production and control of these reflexes. We are not ready to give any detailed report of the results. Our efforts have been confined rather to the general features of the method. We find little in the literature upon such important points as:

(1) Technique of method;
(2) Subjects upon which the method may be used;
(3) Present range of application of method.

(1) *Technique of Method.*—As Bechterew's students affirm, we find that a simple way to produce the reflex is to give a sound stimulus in conjunction with a strong electro-tactual stimulus. Bechterew's students use the reflex withdrawal

[1] I wish I had time here to develop the view that the concept of the conditioned reflex can be used as an explanatory principle in the psychopathology of hysteria and of the various "tics" which appear in so-called normal individuals. It seems to me that hysterical motor manifestations may be looked upon as conditioned reflexes. This would give a *raison d'etre* which has hitherto been lacking.

of the foot: the subject sits with the bare foot resting on two metal electrodes. When the *faradic* stimulation is given the foot is jerked up from the metal electrodes. The movements of the foot are recorded graphically upon smoked paper. We modified this method slightly in our first experiments. We found that the reflex appeared more surely and quickly if the subject lay on his back with his leg raised and supported by a padded rod under the knee. This position leaves the muscles of the lower leg in a more flexible condition. As a further modification we placed one electrode having a large surface under the ball of the foot and a second electrode only one sixteenth of an inch in width under the great toe, and then strapped down the foot across the instep. When the electrical stimulation was given the great toe was raised from the narrow metal strip (toe reflex). This device made the recording of the reflex somewhat easier. While the use of the foot is fairly satisfactory it is inconvenient for general laboratory work. We found that the reflex appears in the finger as readily as in the toe. So satisfactory and convenient is this last method that we have adopted it in all of our later work with human subjects (Figs. 2 and 3). A bank of keys is provided which enables the experimenter (he is in a different room, of course, from the subject) to give at will the sound of a bell coincidently with the current, or separate from the current. In beginning work upon any new subject we first sound the bell alone to see if it will directly produce the reflex. We have never yet been able to get the reflex evoked by the bell alone prior to the electro-tactual stimulation (Plate I, a). We give next the bell and shock simultaneously for about five trials; then again offer the bell. If the reaction does not appear, we give five more stimulations with the bell and current simultaneously—etc. The conditioned reflex makes its appearance at first haltingly, *i. e.*, it will appear once and then disappear. Punishment is then again given. It may next appear twice in succession and again disappear. After a time it begins to appear regularly every time the bell is offered. In the best cases we begin to get a conditioned reflex

after fourteen to thirty combined stimulations (Plate I, b). We have found several refractory subjects: subjects in which even the primary reflex will not appear in the toe when the current is strong enough to induce perspiration. Whether this is due to atrophy of the toe reflex through the wearing of shoes, or to some other cause, we have never been able to determine. In such cases, however, we can rely upon the breathing which we record simultaneously with the reflex toe or finger movement. The breathing curve is very sensitive and a conditioned reflex appears very plainly upon its tracing.

Some General Characteristics of the Reflex

It is interesting at this point to treat of certain characteristics of the reflex. *First*, as regards the similarity and difference between the conditioned reflex and the primary reflex upon which it is grafted. However much they may differ so far as the central nervous pathway is concerned, the general and coarser motor features are closely similar. One watching the movements of a subject first beginning to show a conditioned reflex cannot tell whether he is being stimulated by the bell alone or by the bell and punishment combined. The conditioned motor reflex is usually sharp, quick, and widespread, the whole body as a rule being brought into the reaction at first. Gradually the reflex becomes more circumscribed. This appears clearly in Plates II and IX.

Second, as regards persistence of the reflex; after the reflex has once been thoroughly established it carries over from one day's experiments to the next for an indefinite period. Sometimes a single punishment at the beginning of a day's work is necessary to cause the reflex to make its appearance. We are not able to state over how long a period of time the unexercised reflex will persist. In one case we trained one subject thoroughly in May to the bell, then did not test him again until October. The reflex did not appear on the first ringing of the bell alone, but after the first administration of the combined stimuli (at which the subject disrupted the apparatus although the induction shock was very weak) the conditioned reflex appeared regularly to the bell alone.

Third. We had hoped to make some statements concerning the reaction times of the fundamental and the conditioned reflex. While we are at work upon this problem, we are not ready to make any report as yet.

Fourth. We know that the conditioned motor reflex can be made to undergo reinforcement and inhibition by factors such as those Yerkes has made us so familiar with in his work on the mutual relations of stimuli in the reflex movements of the leg of the frog. A few examples of the rôle such factors play in the control of the reflex may be of interest. Take first the fatigue of the reflex. A well trained subject will react regularly for an indefinite period of time to a stimulus given at an interval of four to five seconds. If now we give the stimulus, *i. e.*, the bell, every two seconds for a short time, he may react for the first three times and then fail. If the interval is then lengthened, or a rest period introduced, the reflex will again appear.[1] It will be seen later that we utilize this principle of fatigue in setting up differential reactions. Oftentimes before the conditioned reflex is thoroughly set up, it will, after a time, begin to decrease in amplitude. Whether the time is increased is not known. When the reflex is beginning to vanish it can be strengthened in a variety of ways, the most usual way being the introduction of the current, but it can be reinforced also by throwing in simultaneously with the bell some other form of stimulation. I have dwelt at some length upon this subject for fear some might advance the view that the conditioned reflex is nothing more than the so-called "voluntary reaction." The fact, in addition to those cited above, which makes such a view less easily held, is the ease with which the conditioned motor reflex can be set up in animals. The strongest argument against such a point of view is the fact that it apparently can be set up on processes which are presided over by the autonomic system. To test this we have made a series of experiments having for their object the establishment of a pupillary reflex by the combined stimuli of a very strong light and a sound (bell). We found that the diameter of the pupil

[1] Plate III, a and b.

under constant illumination with fixation is very steady after the first five minutes; consequently it is possible to make measurements upon the pupil. To ordinary stimulations (sounds, contacts, etc.) there is a slight but not constant change in diameter (at times changes follow evidently upon intra-organic stimulation). But to such stimulation the pupil may respond either by dilation or constriction. In the short time which we had for training subjects we found two individuals in which, after fifteen to twenty minutes' training, the sound alone would produce a small constriction of the pupil in about seventy-five per cent. of the cases. In two subjects no such reflex could be built up in the time we had to devote to them.

The use of the pupil is thus not very satisfactory: first because it is very difficult to obtain the reflex in it; second, because, due to the fact that we have to induce the fundamental reflex by light, it is not possible to use light as a form of secondary stimulation; and third, because the method is very uncomfortable for the subject. Indeed the long training necessary to produce the reflex in refractory cases would probably be actually injurious to the eyes. Our interest in establishing a conditioned pupil reflex was entirely theoretical.

We have also made one brief attempt to establish the reflex on the heart beat; but on account of the fact that respiratory changes show so markedly on the tracing of the heart, we have been unable to convince ourselves that we have produced a genuine conditioned reflex.

Finally, we had hoped to combine this work with the so-called psycho-galvanic reflex in such a way as to produce a method which would yield quantitative results. It seemed a reasonable train of argument to suppose that the sound of an ordinary bell would not cause changes enough in the bodily resistance (or E.M.F.) to produce galvanometric deflections; but on the other hand, that the sound of the bell joined with the faradic stimulation of the foot (punishment) would produce an emotional change sufficient to show. We argued further that if punishment and bell were then given together for a sufficient number of times, the bell alone would come

finally to produce bodily changes sufficient to show on the galvanometer and we would thus have our conditioned reflex. The only fault to be found with such a train of reasoning is that it does not work out when put to practical test. In the first place the bell, as we expected, does not produce observable changes (nor do other ordinary stimuli), but, and this was unexpected, neither does the combined stimulus of bell and electric shock. Violent stimulations such as the bursting of an electric light bulb, burning the subject with a cigarette, tickling with a feather, etc., do, in our set-up (which contains no battery), produce anywhere from ten mm. to one hundred mm. deflection. Furthermore, the movement of the galvanometer does not start until an appreciable time after the stimulus has been given; sometimes not until three or four seconds afterwards (showing that effect is a glandular change). Another difficulty is that after a deflection has been obtained the original reading of the galvanometer cannot again be duplicated (resistance of the body not going back to the same point). It was largely because of these factors that we temporarily discontinued our experiments in this direction.

Method of Using Reflex to Obtain Differential Reactions

As I have sketched the method of using the conditioned reflex it is suitable for working out many problems on reinforcement, inhibition, fatigue, intensity of stimulation necessary to call out response under different conditions, etc. The method, however, has a much wider sphere of usefulness. If we take a subject in whom such a reflex is established to a bell or a light, he will react to any sound or light not differing too widely in physical characteristics. By continued training it becomes possible to narrow the range of the stimulus to which the subject will react. For example, if we train on a given monochromatic light, using red until the reflex is well established, and then suddenly exhibit green or yellow, the reflex appears. The sudden throwing in of the green light will often cause the reflex to fail the next time the red light is given. We proceed then to differentiate the reflex.

As was suggested above we bring about differentiation by punishment with the positive stimulus (red in this case) but never with the negative stimulus (green). The second step in the process of bringing about differentiation consists in exhausting the reflex to the negative stimulus (using the factor of fatigue). This can usually be done by giving the negative stimulus four or five times at intervals of about one to two seconds. After the reaction to the negative stimulus disappears we 'rest' the subject for a few seconds, and then give the positive stimulus. If this procedure is continued long enough the differential reaction is finally perfected. The differential reaction can be so highly perfected that it becomes possible to use it with great accuracy in determining difference limens on human subjects. So far we have tested it out in the fields of light, sound; and contact with very encouraging results (see Plates IV–VII and IX).

As may readily be seen, this extension of the method gives us the possibility of objectively approaching many of the problems in sensory psychology. We give no more instruction to our human subjects than we give to our animal subjects. Nor do we care what language our subject speaks or whether he speaks at all. We are thus enabled to tap certain reservoirs which have hitherto been tapped only by the introspective method. The data which we collect in this way, while they have no bearing upon a Wundtian type of psychology, serve (as far as they go) every practical and scientific need of a truly functional psychology.

(2) *Subjects upon Which the Method May be Used.*—The range of subjects upon which the motor reflex method may be used is wide. We have tried it out in all upon eleven human subjects, one dog and seven chickens.

The method works in a very satisfactory way upon the particular dog with which we worked—a beagle of very mixed breed. In the case of the dog we stimulate the sole of the foot and record the resulting leg movement (Fig. 4). Six out of the seven chicks showed the conditioned reflex in the respiratory curve (Plate II). We failed to get the reflex in one chick. These animals are comfortably saddled with leg

strapped to a punishment grill. The breathing is recorded by means of Rouse's device. Fig. 5 shows the method in use with the great horned owl.

The adult human subjects used were chosen largely but not wholly from among the graduate students of psychology and biology. Three of the subjects used had never had any psychological training. As might be expected the ease with which the method may be used is not dependent upon the previous psychological training of the subject. We give the subjects no instructions or explanations of the purport of the experiment. It is unreasonable to suppose, however, that the adult psychologically trained subjects do not get the drift of what is expected of them as the experiment proceeds. Whether the bodily set or emotion which results from this plays any rôle in the ease with which the reaction may be obtained has not been determined. On the whole I am inclined to think now that students of physics will prove to be our best subjects since they have been trained to make fine observations of small differences in physical stimuli, without at the same time trying to make crude observations of the stimulations arising from the laryngeal or other vocal organs.

Since we began to use the finger in place of the toe we have had only one subject fail to show the conditioned reflex (a graduate student of psychology). This subject also failed to give the conditioned toe reflex. We failed to obtain the great toe reflex (conditioned) upon one other subject, when we first began our work early in the year. We have had not an opportunity of retesting this individual with the finger reflex.

Whether the method can be used widely with children has not been determined. In the course of twenty minutes we obtained the reflex several times upon an eight-year-old boy. When first punished he cried and showed some reluctancy toward having the experiment continue. One of the experimenters then sat in the room with him, and, under promise of a moving picture show after the experiment, the series was completed with smiling fortitude. When once we

get the reflex established thoroughly to the bell, our troubles with children ought to be over, since we can proceed to build up second order reflexes, that is, the bell may be used in place of the electric shock (Plate VIII).

Much to our regret we have not been able during the year to find time to try the method out in pathological cases. We hope that during the coming year we may be able to try the method out thoroughly, especially upon cases to which language methods are not applicable.

(3) *Present Range of Applicability of Method.*—For some time to come the 'reflex method' will be used mainly by the animal psychologists. I shall point out here some of the advantages this method has over the 'discrimination method' now almost exclusively used in studying the sensory side of animal behavior.

As may be easily seen from our description of the technique of the reflex method the secondary stimuli are offered to the subject serially. One of the greatest difficulties in the way of using the 'discrimination method' upon animals arises from the fact that two or more stimuli must be given simultaneously. This in monochromatic light work, to take a single example, is very serious because it calls for very complicated slits, spacing prisms, methods of reversing the positions of the colors, etc. I shall not dwell upon the difficulties of the use of the discrimination method in other sense fields. They are well known. By using the 'reflex method' it becomes possible at once to discard a mass of cumbersome machinery now used both in the manipulation of the stimuli and in the control of the animal. For a complete monochromatic light equipment we shall in the future need a single monochromatic illuminator, a smoked wedge or sector, thermal couple, and galvanometer. This replaces the entire outfit recommended in the Yerkes and Watson monograph. A similar simplification can be made in the apparatus of other sense fields, especially in audition and olfaction.

A great gain is likewise effected on account of the fact that both wild and vicious animals, and animals otherwise

unsuited because of their large size, slowness of gait, etc., may be used. Another distinct gain comes from the fact that the record is made in complete and permanent form by the animal itself. The experimenter ceases to be a factor in influencing the animal's reactions. Possibly the greatest gain comes from the fact that, if our preliminary experiments may be trusted, dependable results may be reached in a fraction of the time required by the discrimination method. The differential reaction to the two bells shown in Plate IX was obtained in the dog after four experiments, lasting twenty minutes each. Had the discrimination method been used it is probable that at least three to five hundred trials would have been required. Since only ten to twenty trials per day can be given by the discrimination method the experiments would not have been completed under fifteen to twenty-five days. A further conservation of time is effected by reason of the fact that a given animal may be used in more than one experiment on a given day. Where food is given after each individual trial, as in the discrimination method, this is absolutely impossible.

At the expense of possible repetition I shall enumerate some of the uses to which the method may be immediately applied.

1. To all forms of experimentation on light, size, form, visual acuity, etc. It is apparently the only method which will enable us to study visual after-images in animals.

2. It is apparently the only existing method of testing auditory acuity, differential sensitivity to pitch, range of pitch, timbre, etc., in any reasonable length of time.

3. It affords us, by reason of the fact that the stimuli may be given serially, a method of testing the rôle of olfaction. We know nothing now concerning olfactory acuity, differential sensitivity to olfactory stimuli, classification of stimuli, the effect of such stimuli on the emotional life of the animal, etc. Nor is it very feasible to carry out such experiments by the discrimination method.

4. The method gives a reliable means of testing sensitivity to temperature and to contact and to the fineness of localiza-

tion of such stimuli—factors which likewise cannot be determined by methods now in use.

When we recall that the reflex method can be used upon man, without modification, in solving many of the above and similar sensory problems, we must admit, I believe, that it will take a very important place among psychological methods. It may be argued, however, that this method is useful only in yielding results upon very simple sensory problems. Although I cannot here enter into the wider applications of the method, I am sure that its field will be a larger and wider one than I have indicated. I feel reasonably sure that it can be used in experimentation upon memory, and in the so-called association reaction work, and in determining the integrity of the sensory life of individuals who either have no spoken language or who are unable for one reason or another to use words—I have in mind deaf and dumb individuals, aphasics, and dementia præcox patients of the "shut in" type. If indications can be trusted the method ought to yield some valuable results on the localization and method of functioning of the various neural pathways.

In conclusion I must confess to a bias in favor of this method. Time may show that I have been over-enthusiastic about it. Certainly I have attempted here to evaluate a method which possibly cannot be evaluated properly until many investigators have had opportunity to subject it to prolonged tests.

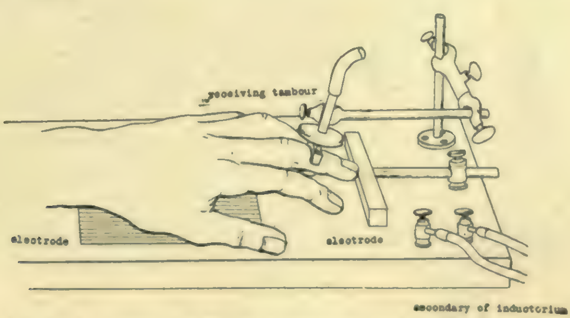


FIG. 2. Method of recording finger movement and of giving faradic stimulation. A large electrode is placed under the hand, and a small electrode under the finger. When key, in the experimenter's room, is pressed down by the operator the secondary current from the inductorium causes the finger to rise from the small electrode. A receiving tambour, to the face of which a saddle-shaped button has been shellacked, enables a graphic record to be made of such movements.

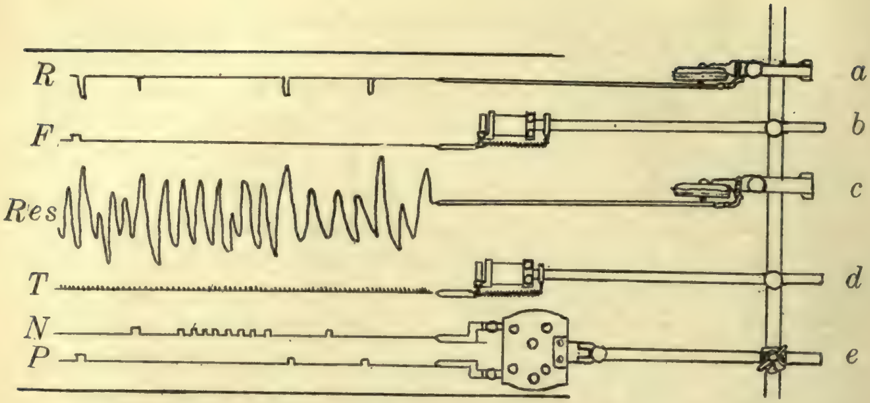


FIG. 3. Showing system of making records. *a*, Marey tambour connected with the receiving tambour from the finger (see Fig. 2). *b*, electromagnetic signal marker, connected with the key for giving faradic stimulation. *c*, Marey tambour connected with pneumograph. *d*, signal marker connected with seconds pendulum. *e*, double signal marker; one pointer moves when the negative stimulus (stimulus not to be reacted to) is given; second pointer moves only when the positive stimulus is given. The letters on the left refer as follows: *R*, conditioned reflex; *F*, faradic stimulation (punishment); *Res*, respiratory changes; *T*, time; *N*, negative stimulus; *P*, positive stimulus. These letters are used in an unchanging way in the illustrations which follow. A short schematic record of the ordinary curves obtained in the laboratory is shown. The eye should begin at the bottom and read up. The first record shows that the positive stimulus was given, that punishment was given simultaneously with it, and that the reflex occurred. The second record shows that the negative stimulus (different bell) was given, that no punishment was given with it, and that the reflex appeared (undifferentiated reflex). Then followed eight stimulations with the negative bell to produce fatigue to the negative stimulus. After fatigue to the negative bell, the positive bell is given. No punishment was given but the reflex appeared. Then the negative bell was given and no reflex appeared. Then the positive bell was given with the appearance of the reflex (differentiation). It will be noticed that respiratory changes show at every stimulation. Both bells cause a deep inspiration, increased amplitude, and a slowing in rate. When training is continued long enough, differentiation occurs in respiration just as it does in the finger movement (see Plate II, *b*); that is, in a short time, only the positive bell can produce the changes shown in this drawing.

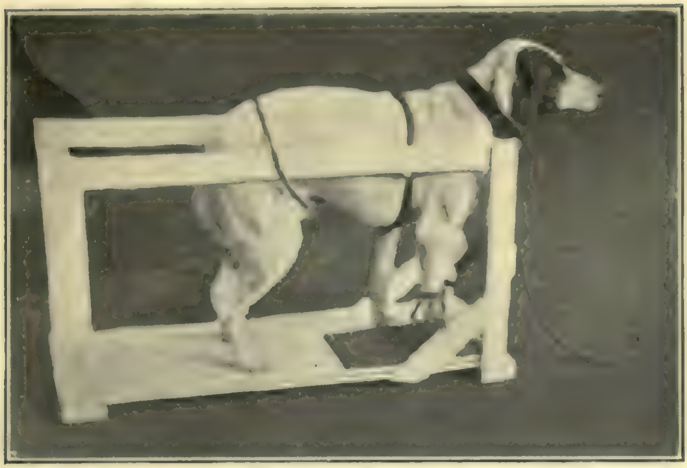
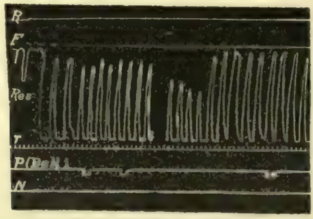


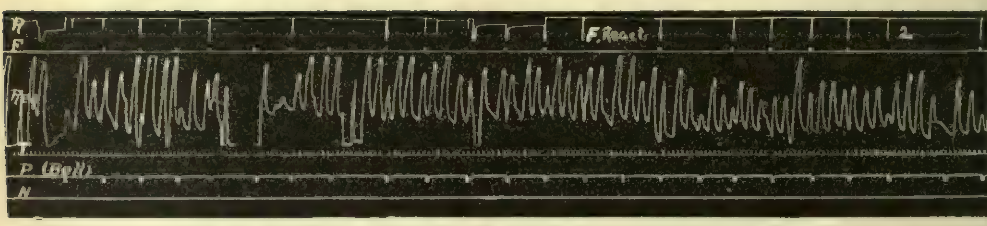
FIG. 4. Shows method of obtaining reflex with the dog. A light spring keeps the foot of the dog pressed down upon the punishment grill. This spring is so light that the dog has no difficulty in pulling up the foot when the faradic stimulus is given. A small receiving device (made like a pneumograph over a coiled spring) or a lever system may be used for the recording of the foot movement.

FIG. 5. Method of obtaining respiratory reflex in all birds. The great horned owl is shown resting comfortably in a padded wooden saddle. Underneath the floor of this apparatus Rouse's respiratory apparatus is shown, sliding on vertical rods. A V-shaped button is shellacked to the receiving tambour, which is adjusted lightly against the bird's chest. The owl's feet are attached to a punishment grill.

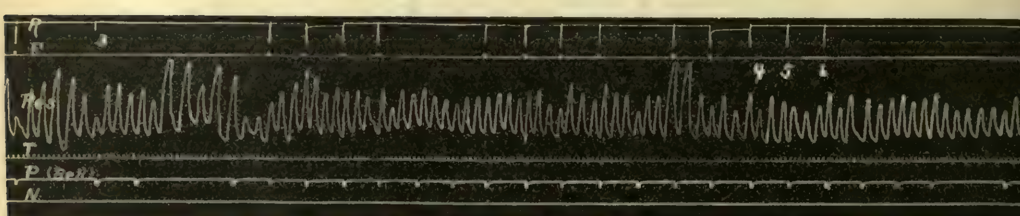
PLATE I. Formation of conditioned motor reflex to sound of bell.



a. No reaction to bell alone.

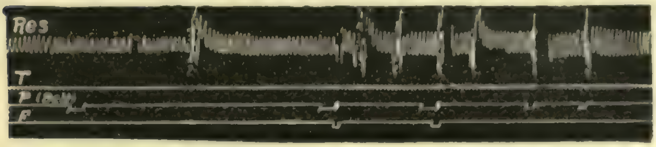


b. Reaction to bell alone (F. React.) after 13 combined stimulations (bell and punishment).

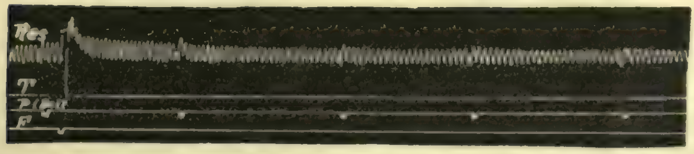


c. Reflex more firmly established as shown by three reflexes, 4, 5, 6, appearing in succession without punishment. (Further training is necessary.)

PLATE II. Conditioned respiratory reflex to sound (bell) in the fowl.

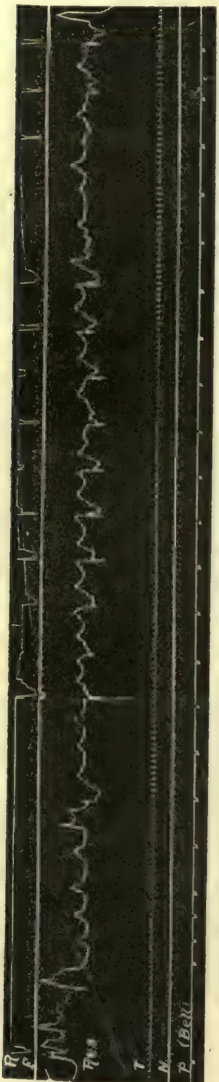


a. Conditioned reflex at beginning of training: Respiration obscured by general motor activity.

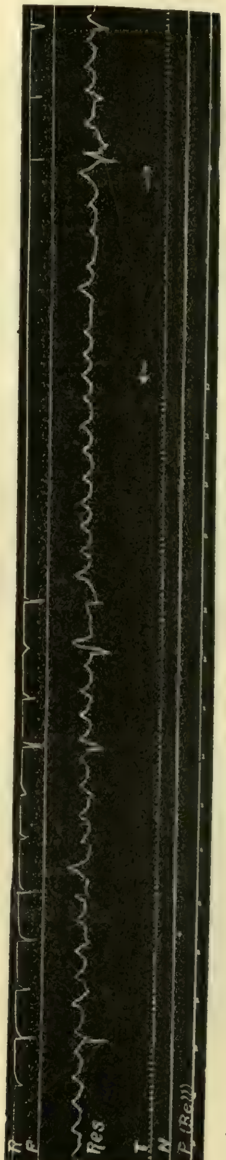


b. Restriction of reflex to respiratory movements after long training.

PLATE III. Reinforcement of conditioned motor reflex. (These records are from trained subjects. Occasionally for one reason or another the reflex will disappear.)

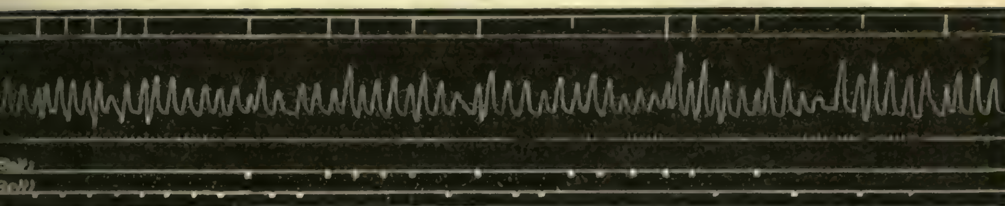


a. Reinforcement by single punishment.

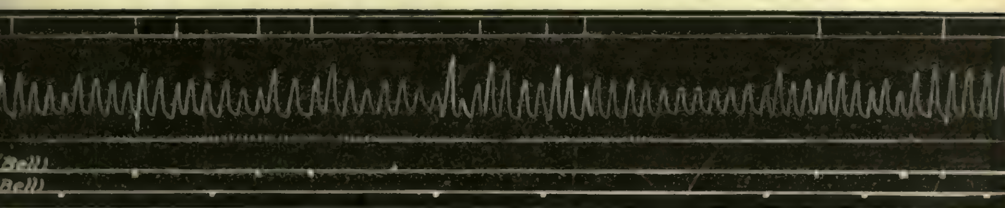


b. Reinforcement by period of rest (between arrows).

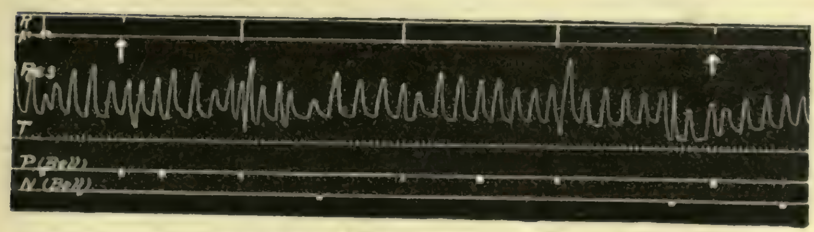
Plate IV. Last stages in the formation of a differential reaction to sound of bells of different pitch.



a. Fatigue of reaction to negative bell.

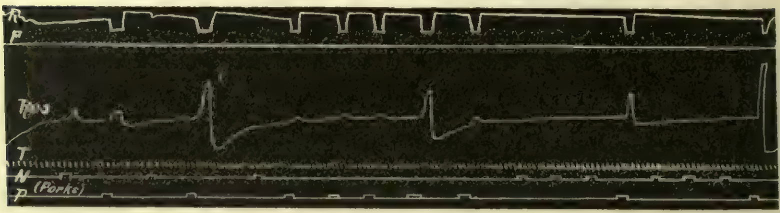


b. Differential not firmly established.

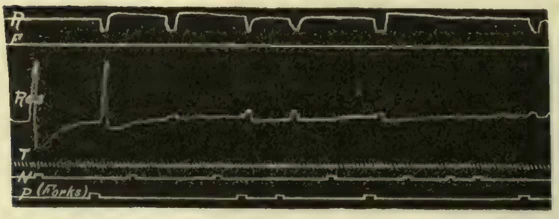


c. Reaction almost perfected after final punishment with positive bell. The arrows indicate that the reflex is present though small.

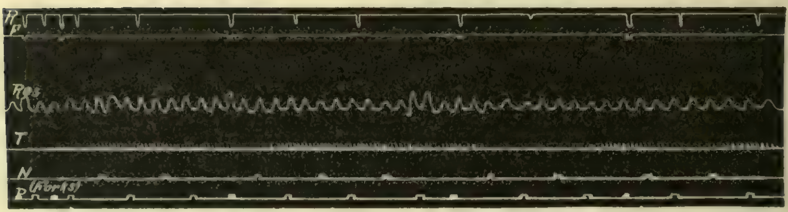
PLATE V. Differential reflex to pure tones. (Standard fork 256 v.d.)



a. Perfect differentiation when the difference is 6 v.d.

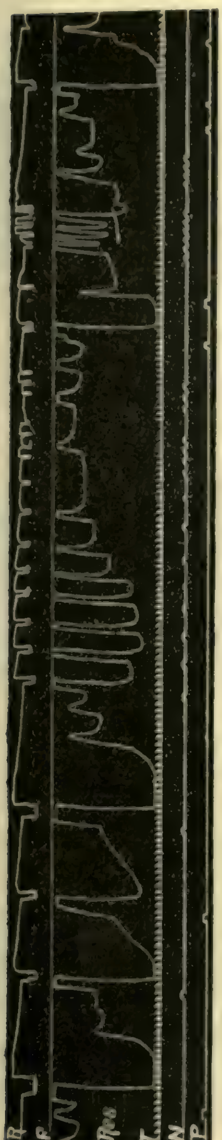


b. Perfect differentiation when the difference is 3 v.d.

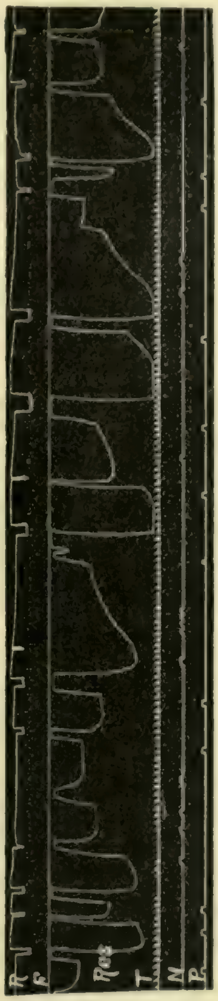


c. Differentiation, in another subject, when the difference is 6 v.d. (This subject failed when the difference was 3 v.d.)

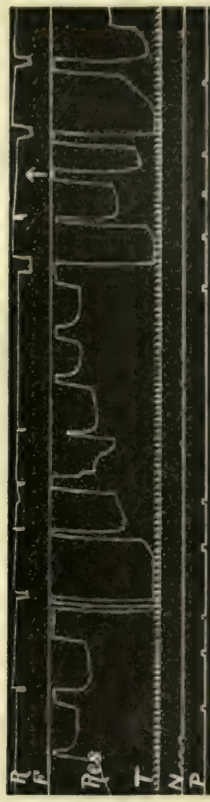
PLATE VI. Differential reflex to two contact stimulations on human forearm.



a. Reflex established but not differentiated: differentiation brought about by fatigue of reflex to negative stimulus.

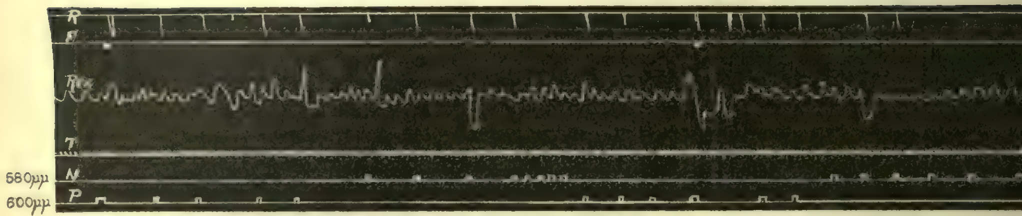


b. Beginning of differentiation.

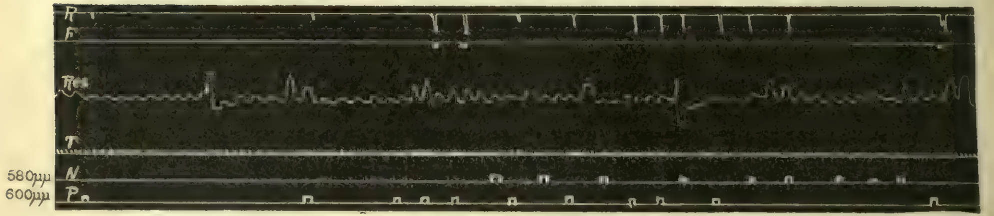


c. Differentiation established (arrow shows that reflex was present in tracing but too faint for reproduction).

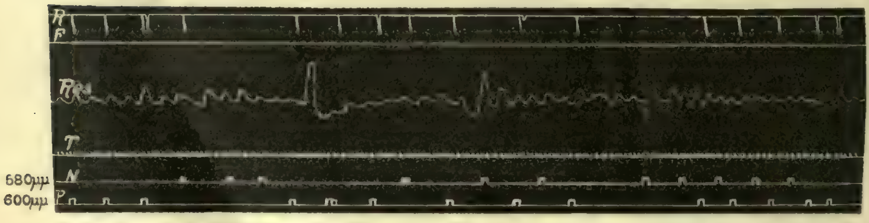
Plate VII. Differential reflex to lights of different wave-length.



a. Reflex established but not differentiated.

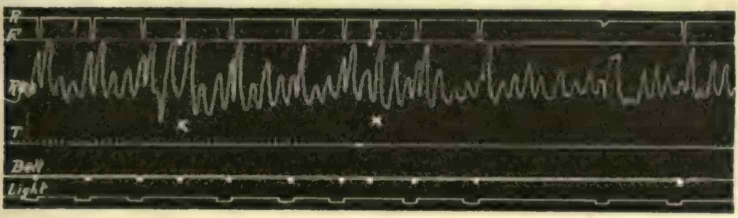


b. Progress toward differentiation. Example of reinforcement by rest.

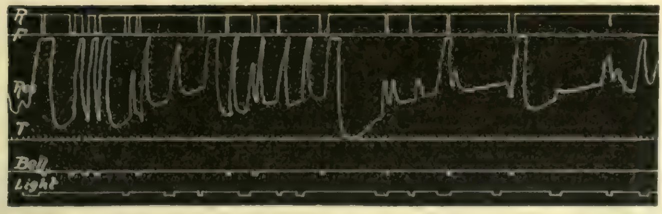


c. Differentiation perfect.

PLATE VIII. Formation of conditioned reflex to light by association with conditioned reflex to sound. (The reflex to sound had been set up previously by the use of punishment.)

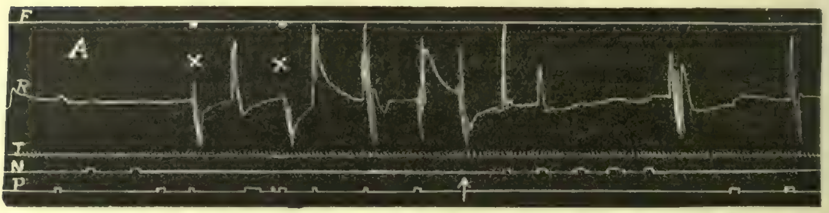


a. Light stimulus combined with sound. Reflex follows with light alone. (*Sound* stimulus *reinforced* by punishment at *x*.)

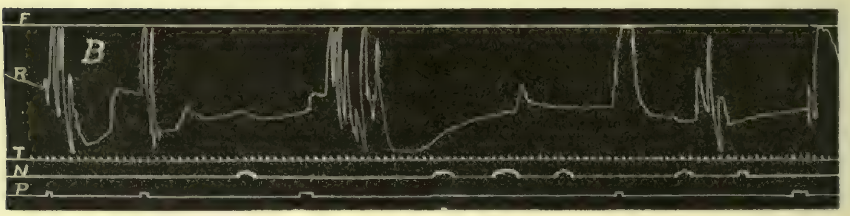


b. Later stages of training. Respiratory reflex well established.

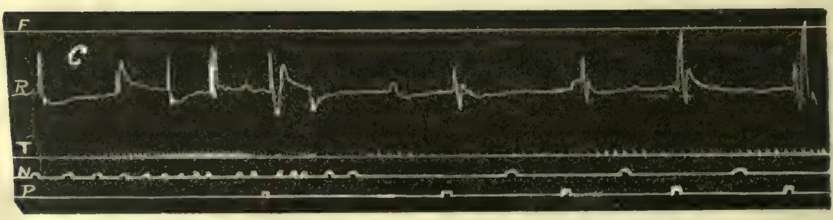
Plate IX. Rise of differential reaction in the dog to two electric bells of different pitch. (*R*, reflex; upward jerk of forefoot.)



a. Undifferentiated reaction after punishment. (Punishment shown at *x*.) Arrow shows rhythmical reaction, no stimulus having been given.)



b. Beginning of differentiation.



c. Differentiation established. (Prolonged stimulation with negative bell until fatigue. This seemed to complete the process of differentiation.)

THE BIOLOGICAL POINT OF VIEW IN PSYCHOLOGY AND PSYCHIATRY

BY E. STANLEY ABBOT

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The advantage and necessity of taking the biological point of view in psychology and psychiatry have been urged in recent years by psychologists and especially by Dr. Adolf Meyer and other psychiatrists. To this necessity the writer heartily agrees. For this point of view makes the study of these subjects objective and as free as possible from metaphysical bias and *a prioristic* theories. It also sets more definite and workable problems, and establishes a criterion for evaluating both old and new theories and conceptions.

Because this point of view seems to the writer to have been imperfectly conceived and inadequately applied, and because it and its implications have been nowhere fully set forth, this attempt to supply the deficiency is made.

If we regard biology only as the science of matter in the living state, it is limited to studies of structure and its physiological activities; but if we take the broader, and in the writer's belief truer meaning, biology is the science of living things. This includes not only studies of structure but of all the activities of the biological unit, including its conduct or behavior. Jennings's studies of the behavior of the lower organisms belong to biology just as much as do Loeb's physiological studies.

The fundamental differences between non-living and living things are that the latter by internal activities *make themselves* out of the materials of their environment, and *reproduce their kind*. In a very limited sense even the non-living thing may be said to *adapt itself to the environment*, in that it reacts by its own inertia to the forces that act upon it, but not by any internal self-directive activities of its own. In the world of living things there is the same reaction of inertia

as in non-living things, but in addition there are internal activities, some of which are of a self-directive nature.

In the vegetable world these internal activities are little, if any, modified by the plant itself, though some botanists hold that root-caps and vine tendrils, for example, exercise a direction of the plant activities which is not wholly explained by the various tropisms to which the plant is subject.

But in the animal series, as we ascend the evolutionary scale, the self-directive internal activities assume a progressively greater prominence in the reactions of the individual to its environment. The life of the individual biological unit consists in the continuous adaptation of itself to its environment as well as it can. That is its job in life. If it absolutely stops reacting by internal activities, it dies. If it does not react as well as it can, it succumbs to external agencies or does less well than its neighbor. (Even we human beings try to justify ourselves and others when we have done less well than we might, and we rarely intentionally do less well than we know at the moment of acting, though often afterwards we see how we might have done better.) This self-adaptation is minimal in the resting stage of spores and seeds, and in the higher animals in hibernation and in sleep, and is maximal in the wide-awake activities of the higher animals, especially man.

We may say then that from the biological point of view man is a biological unit reacting as well as he can to his environment by means of internal self-directive activities which determine his outward acts. Many of these internal activities are physiological, but most of those which result in his external behavior or conduct are psychological. All of his activities are directed to the great end of his best self-adjustment to his whole environment, though lesser or nearer and more concrete ends are usually more immediately prominent to the individual. That is, he usually reacts to the immediate present situation without much thought of his reaction being an integral and essential part of a large adjustment to life, *i. e.*, to his whole environment, including his whole past, present and future situations, experiences and reactions.

The psychical activities of any biological unit, then, are links in the chain of internal reactions by which the unit adjusts itself or reacts to the forces which act on it and the situations in which it finds itself. Each link is a reaction, effect of preceding links, cause of succeeding ones. In psychology then we not only have to study the special psychic act or type of acts as a separate entity or process, but we should always study also its immediate and remote causes and effects. Study of the causes leads back by one route to factors of the environment and by another to anatomical structure and physiological process. Study of the effects leads forward in one direction to behavior, *i. e.*, action in and upon the environment, and in another to bodily changes and processes.

From the biological point of view then every psychic event is a reaction,—a link in the chain, or a moment in the series, of reactions by which the individual adjusts or adapts himself to the environment. The nervous system (or, in the unicellular organisms—if any of their reactions deserve the name psychic, as the writer thinks they do—the body protoplasm and outer membrane) is the specially adapted structure for the performance of the psychic functions or processes. Mind is the abstract name we give (1) to the capacity to react in certain, *i. e.*, psychic, ways, (2) to the organized whole of any individual's psychic reactions, or (3) to the content of any individual's psychic reactions, especially ideational ones. It is in reality a function or set of functions—activity or types of activity—but through misconception it is often used to indicate some mysterious thing which can act of itself or is opposed to or contrasted with body, and it is often referred to as having structure. (See for example, McDougall, 'Mind and Body,' pp. 165, 330.) From a strictly biological point of view it bears the same relation to brain and to the individual that respiration does to lungs and to the individual, or that running does to legs and to the individual. It is the *individual*, not the brain, that thinks or exercises the other psychic activities we call mind, just as it is the individual, not the lungs, that breathes, or the

individual that runs, not the legs. But by means of the brain, the lungs and the legs, the individual thinks, breathes, runs.

We do not think of opposing or contrasting respiration or running with lungs, legs or body. Neither should we do so with brain or body in the case of mind. Similarly it is absurd for us to think of respiration as identical with lungs, or running as identical with legs. It is equally absurd to say, as the identificationists still do, that mind is brain, and brain is mind. To speak of a mind-stuff (except in the sense that we may speak of electrical-stuff or light-stuff on the theory that there is no substance but energy, and the writer has never seen it used in this sense) is equally misleading, and savors of the anachronistic notion that mind is secreted by the brain as bile is by the liver—a notion whose absurdity becomes apparent if we again use the comparison of the lungs secreting respiration, or the legs secreting running.

But this view does not mean that we should neglect the brain or nervous system or body, any more than we should in studying respiration or any other function. It is true that we can study many aspects of psychical as well as other functions with only a slight or superficial knowledge of the organs that subserve them. But in physiological psychology some structural knowledge is essential; in comparative psychology the degree of evolutionary development of the nervous system is seen to affect the capacity for psychic reaction; and child psychology is different from adult psychology on account of the individual development, and it is advantageous to have knowledge of these structural conditions. So intimately are structure and function related that it will doubtless be found eventually that racial, family and even individual traits are partly dependent on more or less minute structural differences in brain architecture and nerve-cell distribution.

In that branch of abnormal psychology which is called psychiatry, in which the scientist has not only to understand the psychological reactions but has to treat the patient also, a knowledge of the brain, nervous system and body in general

is especially needful. The effect on mental processes of such bodily conditions as fatigue, toxæmias (whether from exogenous or endogenous sources), and brain lesions needs only be mentioned, to have its importance recognized. On the other hand, the effects of certain psychical processes, notably some of the emotions, on bodily activities have recently been emphasized by Crile, Cannon and others. In the writer's belief the affects intermediate between the ideational activities on the one hand and the bodily activities on the other, and this action, while usually and most forcibly exerted in the direction indicated, is nevertheless exerted to some extent in the reverse direction—that is, bodily condition acts to some extent on the ideational processes and content through the affects. This is seen especially in the insane. When we see how some of the reactions belonging to two great functions, as digestion and circulation, may mutually affect each other, it should not be a matter for surprise or wonder that some of the reactions of another great function, mind, should affect and be affected by other bodily reactions or functions. From the biological point of view the relations between body and mind are in principle almost as simple as those between body and any other function. The developmental stage of the structure as to both its phylogeny and its ontogeny and the degree of its integrity will determine the capacity of the individual to react in its racial way.

It was said above that in tracing backward the causes of any psychic event we come eventually to the environment. This as a cause of psychic activity has been too much neglected.

It is the work in life of the biological unit to adjust itself to the environment. This means that the environment, or its various factors, must act upon the unit, and that the latter *re*acts to it. Any given unit will react to the extent of its capacity for reacting, and this is determined by its structure. The greater the number of environmental factors to which it can react, the more complete and adequate will be its response. It is the nature of the environmental factors that has determined the types of response of which the unit is capable.

What are the great types of factors and how are they arranged in the environment?

I need not dwell on *matter*, which we call substance, and the *modes of energy* which we call light, sound, heat, etc., as such types of factors which are met with in the concrete materials and forces of the world we live in.

But as factors there are also other *living creatures*, with their *behavior*, and the *content* of their psychic processes, their *thoughts* and *feelings*, especially in man and the higher animals. Predatory animals and their prey, for example, both have to react to the respective desires and behavior of the other. The content of thinking and emotion in man, whether stored in literature or expressed in speech and action in the presence of the responding human, are large factors to which he must respond. They are outside of him, actually existent in the environment. His own past experiences, including his hopes, ambitions and decisions for his future, are actual former occurrences to which he may react in the present, not only by recollecting them but in many other ways, including the predetermination of many future reactions.

Relations are also objective existences to which we human beings at least react. These are not only spatial and temporal, but of inherence, as of the thing in its kind; genetic, as of offspring to parent; social, as of husband to wife, or individual to community; business, as of debtor and creditor; and innumerable other kinds. There has been evolved, in man at least if not in other animals, a capacity to be more or less sensitive to such environmental factors. We need further study of the mechanisms of our sensitivity and reactivity to them.

Similarly, there is *law* or *necessity* as a factor or group of factors. The sword of Damocles *must* fall if the thread breaks; the walls and roof of the subway must fall in if they are insufficiently shored up; etc. We search for innumerable physical, chemical, genetic and other laws or necessities, and we see them constantly operative in the world about us. It is because they actually exist objectively that there has been evolved in us the capacity for responding to them, not

only as being subject to them, *i. e.*, compelled to obey them, but as being sensitive to them, comprehending them. Psychology will benefit by a study of them as stimuli as well as of the mechanisms by which we respond to them.

Further, there are *obligations* and their reverse *rights*, which are objective existences, at least for us human beings. Whenever two men stand in the relationship of debtor and creditor, there is the obligation of the one to pay, the right of the other to be paid; in the parent-offspring relationship there is the obligation of the parent to protect the offspring till the latter is able to lead its own independent existence, and the right of the offspring to such protection; in the relationship of man and other animals to the respective life activities of others some of us recognize the right of all to live their lives as they please provided they don't interfere with others' rights, some of us recognize such rights only in man, some of us only in white men, some of us only in the dominant white man, or superman, etc., and each of us sees his own obligations according to the limits of his vision. It is the part of ethics to study what those and many other rights and obligations are, of psychology to study the mechanism by which we are sensitive and responsive to them. But psychology cannot adequately study the mechanism without a knowledge of the nature of the stimulus any more than physiology can adequately study the mechanism of digestion without a knowledge of the composition of food-stuffs.

Such, to the writer's mind, are the great types of environmental factors. How are they arranged in the environment?

Every biological unit is not only in an environment, consisting of these factors, but each one is at the center of his own environment, and is himself a part of it. It may be regarded as consisting of a set of concentric circles or spheres, each representing a limited situation, the factors of which act with greater or less force upon the unit at the center, and to which the unit responds with more or less activity, physical and psychical. The limits of the circles are very indefinite, one shading into the other. To illustrate, let us consider a patient in a hospital for the insane. Any con-

ceivable situation in life would serve, but abnormal conditions sometimes help to illuminate normal ones. What is his environment?

He is himself at the center of it, his own body, thoughts and feelings being part of it. Next exterior are the room, its walls, windows, doors, the furniture, the warmth, the daylight or lamplight, the people moving about, etc. If the patient is clear, these may be correctly responded to; if he is confused or hallucinated, they may not be. In the next larger circle people speak to him or to each other. He sees books and newspapers about. He may understand what is said or printed, or he may not. Then there are the uniformed women (nurses) and doctors and patients, each having a different relation to each other, the place and to him. He may comprehend these relationships, or may not, in which case he will be puzzled and perplexed. He may recognize the nurse as a nurse, the doctor as a doctor, and be able to call them by name, yet not recognize that he is a patient, that he is in a hospital, etc. He recognizes the terms involved in the relationship, but not the relationship itself. He can see and recognize 2 and 3 but cannot put them together to make 5.

A wider circle of his situation is that he is sick and in need of care—some of his mental activities are modified, his behavior has been different, he cannot control his thinking or his feeling or his conduct as usual. He may have recognized this and come of his own accord to the hospital, or he may not have recognized it, and others had to bring him against his wish and opposition.

Next, he is a business man and a father of a family. He may realize his obligations to his business associates, customers, creditors, and to his wife and children, or not. They and a host of related conditions form parts of his situation.

He may have done, or think he has done things in the past—such as acquire some venereal disease—which modifies his present condition or thoughts. One can go on to show indefinitely wider spheres of environment.

This illustration is not intended to show the limits of the

widening circles of environment, for there are no clear-cut ones, but only to indicate that the environment may be regarded in that way. In comparative psychology and in psychiatry we need to know how large is the environment and to what kinds of factors in it the individual we are studying is capable of reacting.

The factors of the inner circles of the environment, the immediate surroundings, mostly concrete, are constantly changing and shifting, requiring constant adjustment on the part of the individual. The remoter ones as a rule change less, somewhat in proportion to their remoteness or abstractness, though not absolutely so. They all have their influence and effect on the conduct of the individual, through the psychological processes involved in his perception and comprehension of the various factors of the whole situation and of their relative importance at the moment of acting. Other psychological activities of course enter into the final behavior or reaction to the environment, such as various affects, plans, decisions, will-impulses, etc.

In psychiatry especially it is necessary to take into account, not only the patient's psychical reactions, but all the bodily conditions that may modify them, and particularly all the environmental factors both immediate and remote, that make up the successive situations in which the patient is and has been. It is in this field that Freud and his followers have taught us so much. It is partly due to that influence that we are realizing more and more that the aim of the psychiatrist should be to study the patient's *total reaction to his total environment.* The biological point of view makes it insistent however that the study of this total reaction include that of the somatic factors which may modify it, a point that is apt to be somewhat neglected on account of the interest of the psychical reactions. Psychiatry becomes not only a medical science, but an intensive individual psychology as well.

The biological point of view—that every psychic event is a reaction of an individual—if consistently followed and applied, will correct a tendency, prevalent to some extent in

most if not in all psychologies, very common in James's psychology, and fairly running riot in the writings of the Freudian school, to personify, as it were, or to make independently acting entities of, the psychical functions. For example, Yerkes,[1] describing an effect of habit, writes that while talking with his friend, ". . . the series of acts gets itself performed . . ." Münsterberg[2] says: "Each man lives in the world which his inner dispositions select and shape." Baldwin,[3] writing of association, says: ". . . a group of processes, . . . conspire, so to speak, to 'ring up' one another," and he subsequently speaks of the 'conspiracy' and the 'conspiring elements.' James says: "The psychologist gets to supposing that the thought of an object knows it in the same way that he knows it,"[4] and 'the thought that thinks it,' and 'thoughts . . . know objects,'[5] etc., while Freud notoriously introduces a 'censor' and speaks of libido, of dreams, dream-wishes, and various thoughts, affects and abstractions as though they were endowed with independent initiative and activity. Making all due allowance for a proper use of analogies and of abstractions to avoid descriptive phrases and periphrases, and for literary leavening of an otherwise perhaps heavy dough, there yet remains enough of such usage to indicate a haziness of conception on the part of the writers, and to becloud for the reader a subject not too clear at best—not to mention its scientific inexactitude.

For the same biological point of view the scientifically sterile conception of a 'stream of consciousness,' with the pseudo-problems that it raises, does not exist (except as a fancy), any more than an analogous conception of a 'stream of respiration' could exist. In sleep the organism reacts psychically to a slight degree, in unconsciousness it ceases to react at all in those ways, though it continues to react in most physiological ways. The unity of the 'ego' is deter-

[1] 'Introduction to Psychology,' p. 383.

[2] 'Psychology, General and Applied,' p. 221.

[3] 'Mental Development,' p. 266.

[4] 'Principles of Psychology,' Vol. I., p. 196.

[5] *Ibid.*, p. 197.

mined by the facts that it is the same organism which reacts at successive times, that each experience is recorded in the same individual (not in any other), and that the organism can recall the content of most of these experiences by subsequent psychical activities. The partial or split personalities —wrongly called multiple personalities—are partly comprehensible on the grounds that the individual, through mechanisms of which we know little, can not recall and make use of large groups or sets of experiences, and can react in more than one way at a time.

This point of view may seem to be a purely mechanical one, and hence fatalistic, leaving no room for choice or free-will. In the sense that every act, physical, physiological or psychical, has its determinants and its effects, that no reaction is haphazard or occurs by chance, this is true. But that it leaves no room for choice is not true. Every individual, from lowest to highest, is always and inevitably in a situation, some of the factors of which are constantly changing more or less. The individual *must* react to the environment, even if only by inhibiting all external reactions. But there are many possibilities of reaction (more such the higher the individual is in the evolutionary scale), and the individual can, even must, choose which of the possibilities to carry out. The choice itself, the act of choosing, is a part of the reaction to the situation. The choice is but one of the determinants of the act chosen. There is then a compromise between free-will and determinism. The individual *must* react, but has a measure of choice—freedom of will—as to *how* it shall react, *i. e.*, as to *what* reaction it shall make.

Thus it may be said that from the biological point of view man is a biological unit reacting to his environment; that his reactions are partly psychical and partly physical or physiological; that every psychical event is a reaction; that the organized whole of the psychic reactions, or the capacity to react in psychical ways, or the content of the psychic reactions is mind, which is related to body as function or activity is related to structure; that to understand fully the psychic reactions it is desirable and in some instances neces-

sary to know the structure which subserves the function, the modifications of this structure, and the causes which modify it (and hence the function which it subserves); that it is also necessary to know the nature of the various factors of the environment and their grouping in a situation when considering the psychology of any individual's behavior or reaction to the situation; that in psychiatry we must seek to learn the patient's total reaction to his total environment.

SOME LOGICAL ASPECTS OF THE BINET SCALE[1]

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PART I.

PURPOSE

The purpose of this article is,

1. To show how to determine the per cent. of children who should pass a test of intelligence at the age for which it is to be considered standard.

2. To show how the tests could be standardized more accurately than merely by the consideration of the per cent. of children passing it at the age for which it is to be considered standard. This may be done by a consideration of the per cents. of children which pass it at several different ages.

3. To show three criteria of a test as a test of intelligence.

4. To suggest a method of constructing an absolute scale of intelligence and to consider the proper spacing of tests on the scale.

5. To suggest an application of the term "intelligence quotient" such that it shall be constant for any individual throughout the growth of intelligence.

THE DETERMINATION OF THE PER CENT. OF CHILDREN WHO SHOULD PASS A TEST AT THE AGE FOR WHICH IT IS TO BE CONSIDERED STANDARD

In regard to the per cent. of correct responses necessary for locating a test in the Binet Scale, Dr. Terman says:[2] "This is a mooted question. Binet's standard, also that of Terman and Childs, was a shifting one, varying from sixty to ninety per cent. according to the upward character of the

[1] I wish to express my thanks to Dr. Lewis M. Terman for suggesting this study, for supplying needed data, and for making many helpful suggestions and criticisms.

[2] Dr. Lewis M. Terman, 'Suggestions for Revising, Extending, and Supplementing the Binet Intelligence Tests,' *J. of Psycho-Asthenics*, 1913, 18, 24.

curve for the test in question. Goddard, Kuhlmann and Bobertag, on the other hand, adhere strictly to the seventy-five per cent. standard. Bobertag considers at length the justification for this rule, and comes to the conclusion that no definite proof of its correctness is available. The fact, however, that this standard gives us a distribution of mental ages for children of each group age closely approximating the so-called 'normal curve of distribution,' is, in the opinion of Bobertag, a weighty argument in its favor." Kuhlmann, however, on the basis of data which he later gathered from tests of 1,500 normal children, rejects the seventy-five per cent. theory and like Terman and Childs adopts a sliding standard ranging from about seventy per cent. at the lower end of the scale to about fifty-five or sixty per cent. at the upper end.

This lack of uniformity as to the proper per cent. for standardization appears to be due to confusion concerning the logic involved in the derivation of this per cent. from a definition of standard intelligence. The following passages will indicate (1) how standard ten-year intelligence, for example, is to be defined and (2) that from this definition the per cent. for standardization is to be derived. Terman says:[1] "Accordingly our search should be for a standard . . . which would reveal the true median intelligence quotient for non-selected children at each age. What we really want to know about a given child is how he tests with reference to the median child of his years, rather than whether or not his intelligence is exceeded by that of ninety per cent., seventy-one per cent. or sixty per cent. of the children of his age-group."

It might be well at this point to get a clear idea of what is called a 'normal' or 'symmetrical' distribution. Suppose we had the exact measures of the heights of a large group of unselected ten-year children. If a dot were placed at a height above a horizontal line corresponding to the height of each child, these dots would appear as shown at *A* in Fig. 1. If we drew a series of vertical lines representing the appearance of the children arranged in a row in the order of their heights, these would appear as shown at *B*. We would find that the dots were thickest near the middle and consequently an imaginary curve touching the tops of the heads of the children would be most nearly horizontal near the middle. We would find that as we recede from the middle in either direction the differences in the adjacent values of the heights become

[1] *Loc. cit.*, p. 26.

continually larger and that consequently the slope of the imaginary curve touching the tops of the heads of the children would become continually steeper. This form of distribution is called a normal or symmetrical distribution.

If we knew the exact values of the intelligences of a large group of ten-year children, it is quite likely that we would find them distributed in much the same manner, in which case our figure would represent this distribution also, the vertical lines representing the degrees or amounts of intelligence possessed by each. The distribution of the intelligences of nine-year children would, in all probability, be of the same form as that of ten-year children though of a slightly narrower range vertically, and that of eleven-year children the same but of slightly greater range, etc. In order to have a single representative value of ten-year intelligence we have defined it as the median value of the intelligences of all ten-year children. The median value of the intelligences of any 1,000 unselected ten-year children would doubtless be very close to the theoretical standard, and may be used as that standard for working purposes.

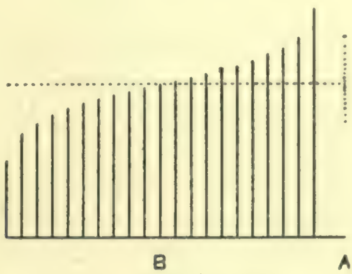

FIG. 1. Showing a hypothetical curve of distribution of intelligences for a single chronological age (ogive form).

Stern says:[1] "The controlling principle for the adjustment or standardization of age-levels is that approximately symmetrical distribution of mental ages must prevail for each level. That is, the tests are properly arranged and skillfully assembled into a system if, when a large number of normal, unselected children of a given age are tested, a large middle group stand 'at-age' and the rest are divided fairly equally between advanced and retarded cases." From these passages it will be seen that standard ten-year intelligence is to be defined as the intelligence of the median ten-year child. It would seem that the logic involved in the determination of the per cent. of ten-year-olds which should pass a test in order that it should be considered a standard ten-year test

[1] Wm. Stern, 'The Psychological Methods of Testing Intelligence,' Warwick and York, 1914, p. 100.

is embodied in the following three propositions, which will hold equally well for all ages:

1. *Standard ten-year intelligence is defined as the intelligence of the median ten-year child.*

2. *Therefore standard ten-year intelligence is exceeded by 50 per cent. of ten-year-olds.*

3. *Therefore a ten-year test will be passed by 50 per cent. of ten-year-olds.*

Possible Objections to the Above Reasoning.—There can be no question that the second proposition follows from the first. Concerning the third, however, it may be urged that a child quite often passes a ten-year test though his intelligence is below ten-year intelligence; it may not follow that 50 per cent. of ten-year-olds will pass a ten-year test even though standard ten-year intelligence is exceeded by just 50 per cent. of ten-year-olds. To this objection it may be said that if a test is passed by 50 per cent. of ten-year-olds, then by the laws of chance, it will happen as often that a child whose intelligence is above standard ten-year intelligence has failed the test as that a child whose intelligence is below standard ten-year intelligence has passed it, or, stated more accurately, by the laws of chance it will happen as often that a child will have a general level of intelligence above standard ten-year intelligence and a degree of the function employed in the test which is below the level of standard ten-year intelligence as it will happen that a child will have a general level of intelligence below standard ten-year intelligence and a degree of the function below the level of standard ten-year intelligence. Since 50 per cent. of ten-year-olds have levels of intelligence above standard ten-year intelligence, it follows that 50 per cent. of ten-year-olds will also have degrees of the function employed in the test which is above the level of standard ten-year intelligence. Therefore 50 per cent. of ten-year-olds will pass a test requiring a degree of a function equal to the general level of standard ten-year intelligence. Certainly this is a ten-year test.

A second possible objection to the foregoing reasoning is indicated by the passage quoted from Terman which reads:

"The fact, however, that this (75 per cent.) standard gives us a distribution of mental ages for each age group closely approximating the so-called 'normal curve of distribution' is in the opinion of Bobertag a weighty argument in its favor." That is, it may be urged that the results actually show that if the 50 per cent. basis be used, 50 per cent. of ten-year-olds will not be found able to obtain a mental age of ten years or better, tending to show that the 50 per cent. basis is incorrect and that a larger per cent. must be used. At first thought this would seem to be a contradiction between the facts and the logic. Let us suppose, however, that the tests were standardized on the 50 per cent. basis but that when children are tested their score is made too low by some error in the manner of scoring; then, of course, less than 50 per cent. of ten-year-olds would attain a score of ten years in mental age, that is, the distribution of mental ages would not be normal. Similarly, if the tests were standardized on a 75 per cent. basis so that ordinarily 75 per cent. of ten-year-olds would be expected to receive a score of ten years in mental age, this same supposed fault in the method of scoring might reduce the number receiving the score of ten years to 50 per cent. and give us a normal distribution of mental ages for ten-year-olds. Therefore "the fact . . . that this (75 per cent.) standard gives us a distribution of mental ages for each age group closely approximating the so-called 'normal curve of distribution'" is not at all conclusive proof of the validity of the 75 per cent. standard. In fact, the existence and nature of an actual error in the present method of scoring similar to the above hypothetical one will be shown.

A third possible objection to the foregoing reasoning is that a ten-year test is not to be considered as one which in general requires ten-year intelligence to pass it, but is merely such a test that when standardized by the proper per cent. and the series scored by the particular method now in use, the number of ten-year-olds attaining a score in mental age above ten years will equal the number attaining a score below ten years and that the distribution of mental ages of ten-year-olds would be normal with the median mental age

at just ten years. (It is quite possible for the distribution of mental ages to be normal yet with the median mental age above or below ten years.) To this objection it may be stated that, as will be shown, even with ten-year intelligence defined as above, the 50 per cent. method will give the desired results and will be found simpler than the trial-and-error method of obtaining the 'sliding standard' of per cents.

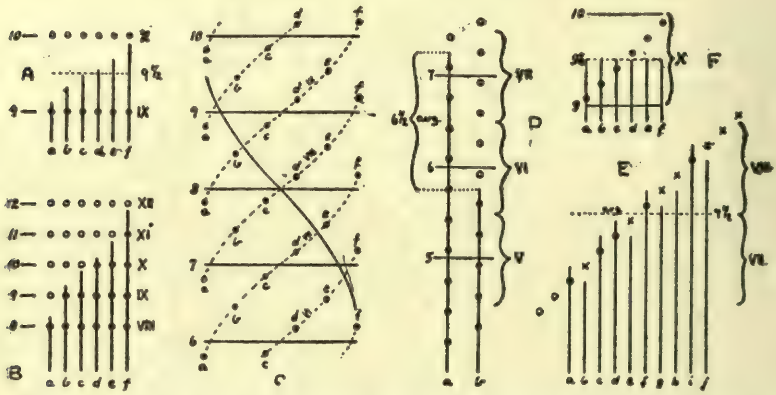

FIG. 2. Showing that by the present method of scoring the mental age of a child may be rated one-half year too low.

The Error of Scoring.—Let us assume (see Fig. 2*A*) that the six tests in group IX. measure six different functions which are represented by the letters *a*, *b*, *c*, *d*, *e*, and *f*, and that the six tests in group X. also measure these six functions. Let us assume that a child passes all the tests in group IX. but fails all in group X. We do not know how far the ability required in any test in group IX. was exceeded by the degree of the function actually possessed by the child (as represented by the vertical lines). The best we can do is to assume that some tests were barely passed while others were considerably more than passed. We should, in fact, assume that the degrees of the six functions tested were distributed according to the law of distribution, as shown in the figure. If so, the general level of intelligence as measured by the average height of the six lines would be 9½-year intelligence, the mental age which of course the child should be accorded.

By the method of scoring now in use, however, the child would be given a mental age of but 9 years.

That this fact is overlooked by Stern, and presumably by many who use a method of scoring involving this error, is suggested by the passage (page 36) which reads: "Considering the problem schematically we might think that the grade of intelligence could be expressed by the stage whose tests could just be passed by the child; a subject who readily passed all the tests up through the 9-year ones, but failed with the 10-year and subsequent ones, would, accordingly, possess a 9-year grade of intelligence."

If we assume the distribution of specific abilities to be wider we may represent it as at *B* in Fig. 2. Here the average height of functions *a* and *f*, of functions *b* and *e*, and of functions *c* and *d* stands at the level of the ten-year tests. Therefore, the child should be given a score of ten years in intelligence, whereas by the method of scoring now in use he is given but $9\frac{1}{2}$ years (8 years for passing all six of the 8-year tests and $\frac{1}{6}$ of a year for each test passed above these, making the score 8 and $\frac{9}{6}$ or $9\frac{1}{2}$ years).

It is very unlikely, however, that the tests are accurately placed even with respect to order. Results of experiment show that the tests in each age group in the Binet scale are far from being at the same level. Allowing only for chance errors, we may assume that the six tests which are supposed to be 8-year tests are in reality distributed on the scale according to the law of distribution, and that the same is true of the other groups of tests. The positions of the six small circles (Fig. 2*C*) on the dotted curves represent the possible degrees of difficulty of the six tests of each group. If it be assumed first that the degree of ability in each function is at a level of 8-year intelligence, we see that the child can pass test *a* in group IX., tests *a*, *b*, and *c*, in group VIII., tests *a*, *b*, *c*, *d*, and *e*, in group VII., and all in group VI. He would therefore receive a score of 6 and $\frac{9}{6}$ or $7\frac{1}{2}$ years (again a half year too little). And if we assume the distribution of specific abilities to be "normal" as represented by the curve, of which the average, median, or mode is 8 years, we

get again a score of $7\frac{1}{2}$ (5 and $\frac{15}{6}$) by the same method as before.

If we assume that the tests were of such character as to test only two functions (*a* and *b* at *D*, Fig. 2), that the tests of each age-group were distributed at fairly equal intervals as shown, and that the degrees of the two functions measured were as indicated by the vertical lines, the child would then pass all the 5-year tests, four 6-year tests, two 7-year tests and no 8-year tests, giving a score of 6 years, while the average of the probable values of the two functions is seen to be $6\frac{1}{2}$ years. Again the score is a half year too low.

Finally, let us assume that each test measures a separate function (Fig. 2*E*; *a*, *b*, *c*, . . .), the height of the circle or cross measuring the degree of difficulty of the test in each function. Supposing those tests passed to be as indicated by the circles and those failed to be as indicated by the crosses, we should, for lack of more data concerning the degree of each function involved, be compelled to assume that it was approximately as indicated by the vertical lines. An average of the probable values of the functions obtained from significant tests would be $7\frac{1}{2}$ years, while the score that would be given under these circumstances is only 7 years. It is therefore quite certain that the method of scoring is in error in that a half year is subtracted from every score.

The Effect of the Error in Scoring.—It is this error which in all probability obscures the error of using the bases other than the 50 per cent. basis for standardizing tests, or has seemed to necessitate this counterbalancing error, as has been suggested. Kuhlmann says: "Taking other facts already brought out into consideration, it becomes evident further that this percentage should vary some with the age group. It should be higher for the lower age groups than for the higher age groups." It would seem to be the aim of Kuhlmann, Terman and others to find a series of per cents. above 50 per cent. such that the "ten-year tests" derived by the use of these per cents. will be easy enough so that when scored by the present method (which is erroneous in subtracting a half year from each true mental age) 50 per cent. of ten-year-

olds will be able to achieve a score of ten years in mental age. The necessity for such a series of per cents. may be obviated in either of two ways, in both of which the single 50 per cent. basis is used throughout. First, one half year may be added to each score as obtained at present, or, that which amounts to the same thing, we may proceed to score as at present except that a child is given a mental age of ten years if he passes tests equal in number to all up to and including *one half* of the ten-year group instead of all of them, etc.; or, second, if it be desired to score the tests precisely as at present, this may be done if those tests are placed in the ten-year group which are standard for ages ranging from nine to ten, thus averaging 9½ years, and if those tests are placed in the nine-year group which are standard for ages ranging from eight to nine, thus averaging 8½, etc. In using the second method it will be seen, as shown at *F* in Fig. 2, that if a child passes one half of the so-called ten-year group he will receive a score of 9½ years which will be correct, and if he passes all the tests of the so-called ten-year group he will receive a score of ten years which will be correct, etc. The only objection to this method is that those tests which are called the ten-year group are standard for ages averaging only 9½ years and hence are not correctly named. This is perhaps of no consequence if the facts are understood. In view of the fact also that it would be difficult to establish a new method of scoring, this latter method of constructing the scale is recommended.

A Consideration of Intelligence as a Function of Age

That ten-year intelligence is relatively closer to nine-year intelligence than nine-year intelligence is to eight-year intelligence is suggested by the fact that there are fewer ten-year-olds who exceed nine-year intelligence than nine-year-olds who exceed eight-year intelligence. And the fact that the number of 17-year-olds, 18-year-olds and 19-year-olds who exceed 16-year intelligence is practically the same as the number of 16-year-olds who exceed it suggests that while intelligence increases with age, the increments decrease with

age, and that after about the age of 16 years the intelligence does not increase at all, or but very little.

If we had curves representing the growth of intelligence in a number of children ranging from below normal to above normal, in which the years were measured as abscissas and the intelligences as ordinates, the curves would probably look somewhat like those in Fig. 3. The dots on the curves

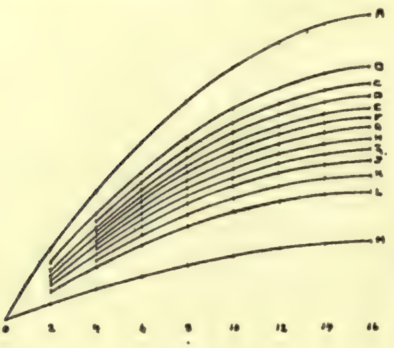

Fig. 3.

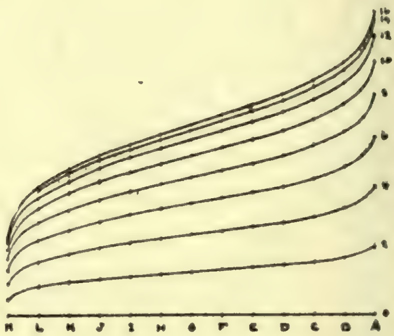

Fig. 4.

Fig. 3. Hypothetical curves of growth of intelligence for individuals of varying degrees of brightness.

Fig. 4. Hypothetical curves of distribution of intelligences at various ages (ogive form).

A, *B*, *C*, etc., represent the tops of the ordinates measuring the intelligences of the individuals for different years of their ages.

If on the other hand we were to consider the individuals placed in order of brightness at equal intervals along the horizontal axis and their intelligences in successive years dotted in one vertical column for each (as shown in the case of individuals *A*, *B*, *C*, etc., in Fig. 4), and if we then drew a smooth curve through the dots representing the intelligences of each at two years, four years, six years, etc., we would have a series of curves as shown in Fig. 4. These curves represent the distribution of mental ages for each chronological age.

The following will serve as an analogy. Suppose runners were arranged in order of ability from left to right and started

together. At the end of say two minutes they would be arranged in a curve similar to the first one above the horizontal axis. At the end of four minutes they would form a curve similar to the second, etc. The distance traversed by any runner would be slightly less during each succeeding minute. To complete the analogy we should suppose the rates of the different runners to be proportional throughout and that they came to a stop at the end of 16 minutes.[1]

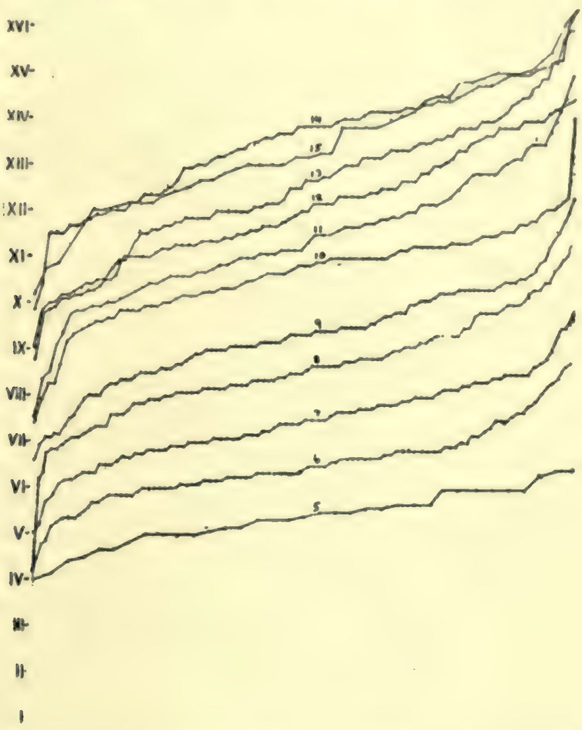

FIG. 5. Distribution of mental ages of a large number of children of various ages as found by Terman.

Curves analogous to these, representing the distribution of mental ages for each chronological age of a large group of elementary school children tested by Terman, are shown in

[1] The curves may be considered also as being made up of an infinite number of points, the heights of those in the same vertical line representing the different degrees of intelligence of a single individual at successive ages.

Fig. 5.[2] In this group the 15-year children appear slightly selected.

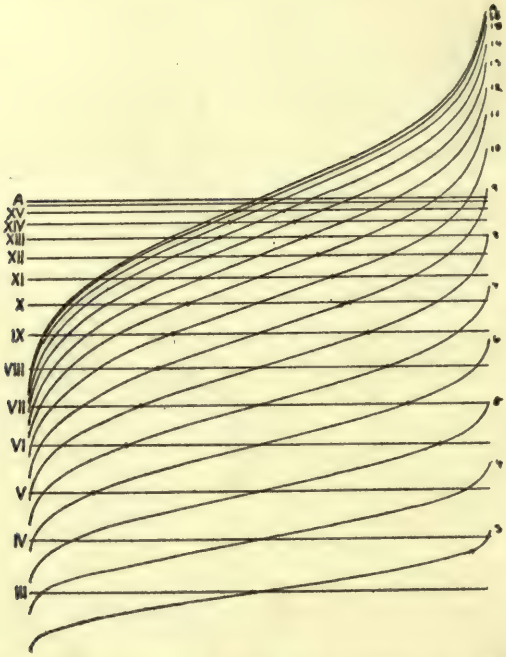

FIG. 6. Hypothetical curves of distribution of intelligence at the various ages showing the levels of intelligence which would be standard for the various ages and the portion of the distribution at each age (shown between the two dots) which would be considered "at-age."

In Fig. 6 we have a complete set of the curves of which those in Fig. 4 illustrate the derivation.[1] Horizontal lines

[2] The height of each dot above the horizontal axis measures the mental age of a single individual. As may be seen, they are arranged in the order of mental ages in each group of a single chronological age, at equal horizontal distances, and then joined by lines. Such a 'curve' or *ogive*, of course, gives more complete data than the histogram in which only the size and extent of groups are given by rectangles. The mental ages are computed on the basis of a tentative arrangement of the tests by Terman and not on his final revision of the Binet scale.

[1] The ogive form of the curve of distribution, of which the equation is $y = ke^{-h^2x^2}$, was chosen in preference to the common bell-shaped curve, of which the equation is $y = \int ke^{-h^2x^2}dx$ for the reason that it admits of greater ease of comparison of a number of curves in juxtaposition. It will be noted that the range of distribution in each curve has been made proportional to its distance above the horizontal axis, or, in other words, the coefficient of dispersion is the same for all the curves. This follows from Fig. 3

representing the standard intelligences at each age are drawn. A dot on the curve of the 10-year-olds, at a point where the curve crosses the 11-year level, marks the dividing line between those 10-year-olds who fall below and those who exceed 11-year intelligence. A dot on the 10-year curve at the 9-year level shows what portion pass the 9-year intelligence level. If we define as 'at age' those 10-year-olds whose intelligence falls between 9-year and the 11-year levels, that proportion of 10-year-olds will be represented by the proportion of horizontal distance between the two dots on the 10-year curve, and similarly for the curves of other years. We see at a glance that fewer are at age in the older groups than in the younger. This is true for two reasons, each self-sufficient: first, because the curves are closer together at the upper ages; and second, because the curves have a greater slant at the upper ages, which represents the fact that the distribution of a certain portion of a group is wider in the upper ages.

Consideration of the More Accurate Placing of Tests

In the building of a tentative scale, it is of course desirable to place each test as accurately as possible, conveniently, as to the age for which it shall be considered standard. This may be done by considering not merely the per cent. of children of one certain age which pass the test but by taking into account also the per cents. passing at several different ages. Thus, suppose the per cents. of children at the ages of 9, 10, and 11, passing a certain test were respectively 44 per cent., 50 per cent., and 74 per cent. By the consideration of the 50 per cent. alone we would immediately say that it was a 10-year test. Now let us suppose that we knew that the per cents. at these three ages which should pass a 10-year test were respectively 32 per cent., 50 per cent., and 63 per cent. Then, judging by the two per cents. other than the 50 per cent., we should be inclined to think that the

in which the assumption is made that the difference in intelligence between two persons of the same age, through successive years, increases in proportion to the increase in the absolute amounts of their intelligences.

test was really easier than a ten-year test and that the 50 per cent. had been accidentally too low. A knowledge of the per cents. of 9- and 11-year children which would normally be expected to pass a 10-year test would therefore be of value for the more accurate placing of the test, and of course, the same would hold for the other tests. Having fixed 50 per cent. as the per cent. of children which should pass each test at the age to which it is to be assigned, it is our problem to determine the per cents. of children which should pass any test at the age of one year more, one year less, and if possible, two years more and two years less than the age for which the test shall be considered standard, etc.

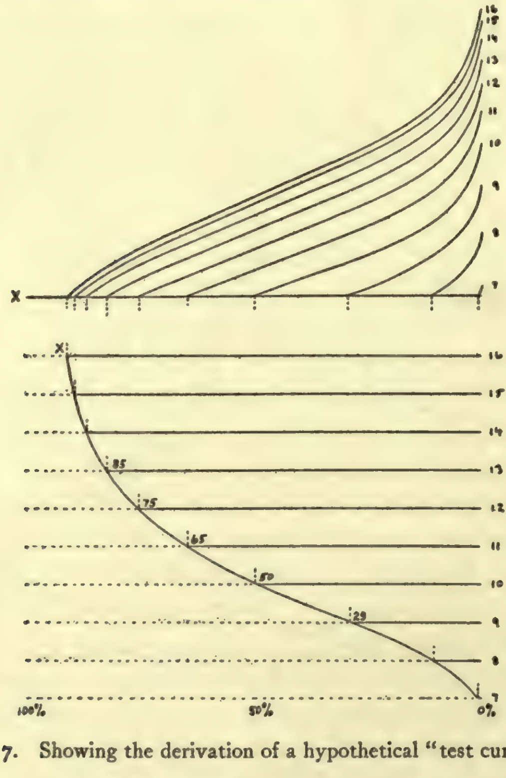

FIG. 7. Showing the derivation of a hypothetical "test curve."

If we examine the intersections at the horizontal line through the median value of the intelligences of the ten-year-olds (see Figs. 6 and 7), it will appear that 10-year intelligence

will be exceeded by perhaps 29 per cent. of 9-year-olds, 65 per cent. of 11-year-olds, 75 per cent. of 12-year-olds, 82 per cent. of 13-year-olds, etc. These would, therefore, be the hypothetical per cents. of children at these ages which would be expected to pass a 10-year test. It will be shown how values of these per cents. may be derived from data.

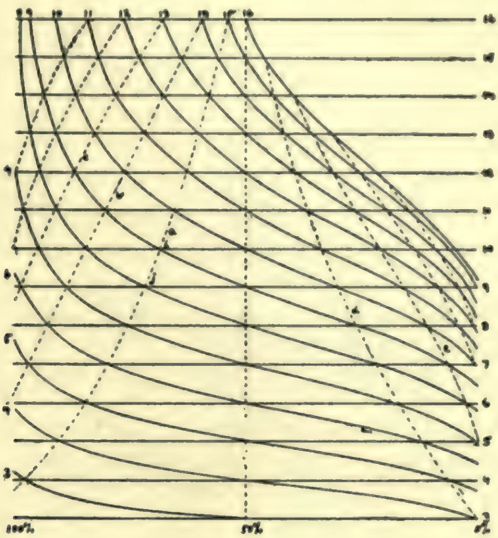

FIG. 8. A group of hypothetical test curves derived as in Fig. 5, showing that the intersections of the test curves and age lines lie in smooth curves.

If on horizontal lines representing the various ages (Fig. 7) we lay off the per cents. of children at these ages which are indicated by Fig. 6 to have intelligences above 10-year intelligence (these per cents. being represented as full lines in the figure),[1] we can draw a curve through these points. In Fig. 8 is shown the whole series of curves of which curve 10 is the same as the curve of Fig. 7. The intersection of each curve with the horizontal age lines marks the per cent. of children of those ages which are indicated by Fig. 6 to have intelligences above that standard for the age represented by the curve and which would be expected, therefore, to pass

[1] For reasons which will appear, it was thought best to measure these per cents. from the right rather than from the left.

a test standard for that age. Intersection (*i*) indicates the per cent. of 10-year-olds who would be expected to pass a 9-year test; intersection (*j*) indicates the per cent. of 9-year-olds who would be expected to pass an 8-year test, etc. It will be seen that the intersections indicating the per cents. of children at each age who would pass the test standard for the age of one year less, lie in a smooth curve (*a*). So do the intersections indicating the per cents. of children of the various ages which would pass a test standard for the age of two years less, etc. Now if we had the true position of these curves (*a*, *b*, etc.), we could draw the true test curves, giving the per cents. of children at the various ages who would pass a test standard for any age. If we had the per cents. of children of various ages which passed certain tests at the age of one year more than the age at which just 50 per cent. passed

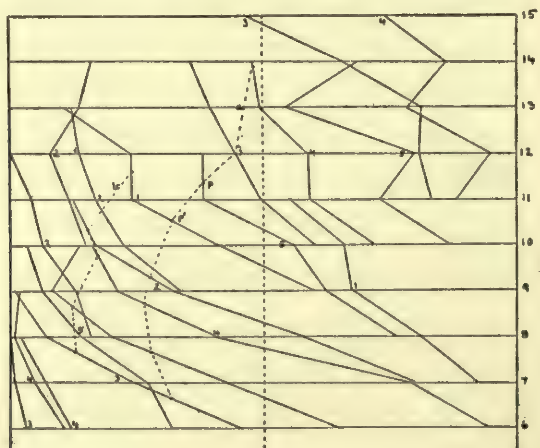

FIG. 9. Some test curves derived from data given by Kuhlmann showing the points p, p', etc., which would indicate the per cents. of children by which it was assumed the tests would have been passed at the age of one year more than the age at which just 50 per cent. would have passed them.

them and plotted these per cents. on age lines as in Fig. 8, it is presumable that these would lie in a position approximating that of curve (*a*) in Fig. 8. A smooth curve drawn then through these points would give us an approximately correct position of this curve.

Kuhlmann[1] gives us per cents. of children at various ages which passed each of the tests of the scale. Certain of these per cents. were plotted in Fig. 9 and the points joined with broken lines analogous to the test curves in Fig. 8. The broken line in the center marked 5, for example, shows that test No. 5 in group X. was passed by 24 per cent. of 8-year-olds, 38 per cent. of 9-year-olds, 44 per cent. of 10-year-olds, 62 per cent. of 11-year-olds, and 62 per cent. of 12-year-olds. In order to make a rough estimate as to the age at which just 50 per cent. would have passed the test, we may note that the broken line crosses the 50 per cent. line at a point representing approximately the age of ten years and four months, and we might assume that this is the age sought. To find the per cent. that would pass the test at the age of one year more than that at which just 50 per cent. pass it, we should look to see at what point the broken line would cut the horizontal line corresponding to the age of eleven years and four months. This point (p) we find to correspond to approximately 62 per cent. The age at which 50 per cent. pass test No. 1 in group XI. is supposedly 9½ years, since the broken line crosses the 50 per cent. line at this point. The per cent. (p') which may be supposed to pass this test at the age of 10½ (one year more) is about 68 per cent. Similarly points corresponding to p and p' were found on the other broken lines and these are joined by the dotted line (a) which corresponds to dotted line (a) in Fig. 8. Similarly, dotted line (b) in Fig. 9 corresponds to dotted line (b) in Fig. 8.

The points on the dotted lines a and b in Fig. 9 were transferred to another plot (Fig. 10) and in addition to these were added dots similarly obtained from the plotting of all other per cents. given in Kuhlmann's table and a large number of per cents. found by Terman and others. The points designating the per cents. of children passing each test at the age of one year above or below that at which it was assumed just 50 per cent. would have passed it, are

[1] 'Results of Examining a Thousand Public School Children with a Revision of the Binet-Simon Tests of Intelligence by Untrained Examiners,' *J. of Psycho-Asthenics*, June, 1914.

indicated by dots; two years above or below by short horizontal lines; and three years above or below by short vertical lines. Smooth curves were then drawn through these groups

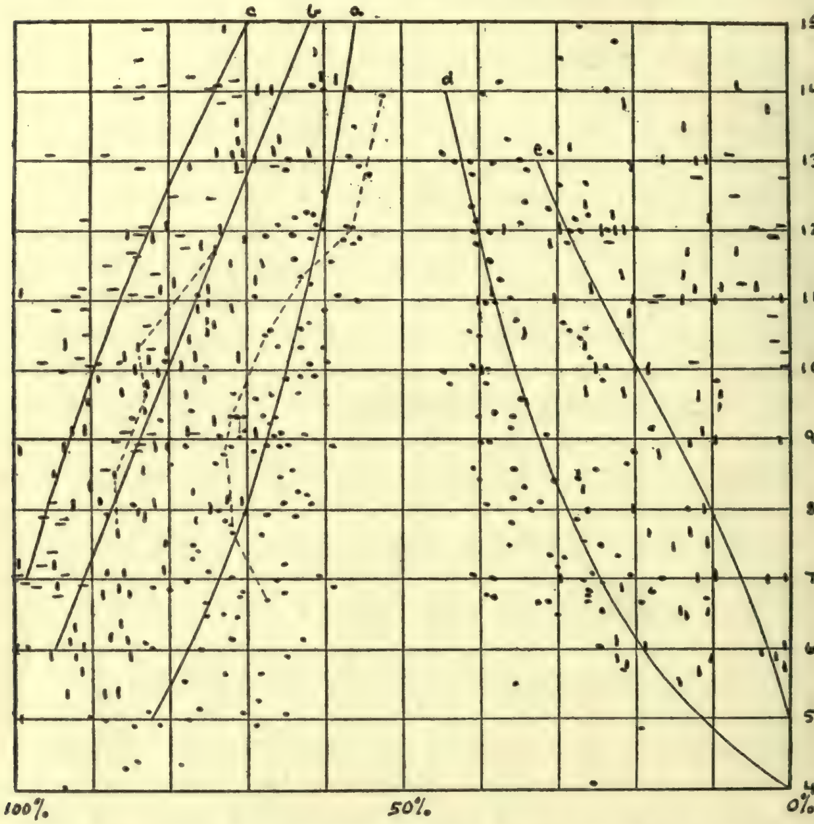

FIG. 10. Showing the derivation of the lines *a*, *b*, *c*, *d*, and *e* of Fig. 6 from data.

of points by the eye as nearly conforming to the points as possible and at the same time bearing a proper relation to one another. This, as may be seen, could not be done very satisfactorily on account of the wide scattering of the points. The wide scattering is due very likely to the fact that the per cents. in many cases were based upon a small number of children. The curves *a*, *b*, *c*, *d*, and *e* in Fig. 10 correspond to curves *a*, *b*, *c*, *d*, and *e* in Fig. 9, the former being hypothetical and the latter derived from data. It will be con-

ceded, I think, that the data are of such character as to support the hypothesis upon which curves (*a*, *b*, *c*, *d*, and *e*) were derived.

The curves *a*, *b*, *c*, *d*, and *e* in Fig. 10 were then transferred to another plot (Fig. 11) and the generalized test curves drawn in. Since the 10-year test curve, for example, crosses the 9-year line at a point representing 32 per cent., we must regard this as the per cent. of 9-year-olds that we should expect to find passing a 10-year test, ideally. Simi-

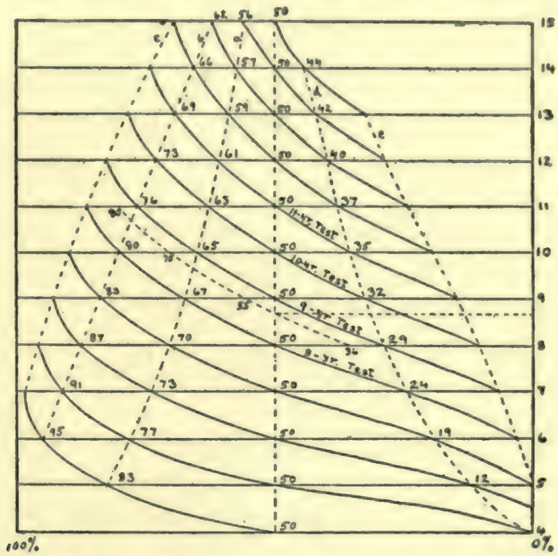

FIG. 11. True test curves as obtained from data.

larly we should expect to find 63 per cent. of 11-year-olds, 73 per cent. of 12-year-olds, etc., passing a ten-year test. It might be said that, as a corollary to the 50 per cent. basis of standardization of tests, a test passed by 32 per cent. of 9-year-olds is a ten-year test, or a test passed by 63 per cent. of 11-year-olds is a ten-year test, etc. A dotted test curve in Fig. 11 crossing the 50 per cent. line at a point representing an age of 8⅔ years is found to cut the horizontal lines at points indicating that a test which would be standard for the age of 8⅔ years will be passed by 36 per cent. of 8-year-olds, 55 per cent. of 9-year-olds, 70 per cent. of 10-year-olds, 80 per cent. of 11-year-olds, etc.

What we desire to know, however, is the age for which a test should be considered standard if passed by *any* per cent. of children at any integral age. For this purpose Fig. 12

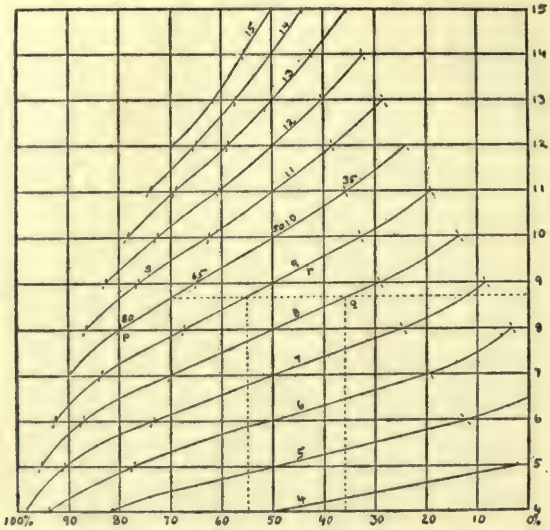

Fig. 12. Diagram for standardizing tests when the per cent. of children passing it at a certain age is known.

was constructed from the data of Fig. 11 in such a way that the curves in Fig. 12 correspond to the horizontal lines in Fig. 11 and vice versa. To find the age for which a test is standard which is passed by 80 per cent. of 10-year-olds, we note that the curve numbered 10 cuts the 80 per cent. vertical line at a point *p* opposite a height marked 8 on the vertical scale, showing that the test in question is an 8-year test. To find the age for which a test is standard which is passed by 36 per cent. of 8-year-olds, we find the point *q* at which curve 8 (representing 8-year-olds) cuts a vertical line corresponding to 36 per cent. (shown dotted) and note that this point is opposite a height on the vertical scale 8⅔ years; therefore the test in question is standard for that age. A test passed by 55 per cent. of 9-year-olds will be seen by the same figure to be standard also for the age of 8⅔ years. These facts correspond to those noted concerning the dotted

curve in Fig. 11. In this way the age for which a test is standard may be found for *any* per cent. of children, not too far from 50 per cent., passing it at any age.

Returning now to the consideration of the test passed by 44 per cent., 50 per cent., and 74 per cent. respectively of 9-, 10-, and 11-year children, we find by examining Fig. 12 that the passing of the test by 44 per cent. of 9-year-olds would indicate (see point *r*) that it is standard for the age of about 9⅓ years, and the passing of the test by 74 per cent. of 11-year children would indicate (see point *s*) that it is standard for the age of about 9⅙ years. Averaging 10, 9⅓ and 9⅙ we get 9½ years as the age which is probably more nearly correct than any of the others, as the age for which the test is standard.

Another method of combining the indications of three per cents. is to add them and find the indication of the sum. For example, the sum of the per cents. of children at the ages of 8, 9, and 10 who are expected to pass a 9-year test by Fig. 11 is 144 (29, 50 and 65). Since the sum of the three per cents. at these three ages passing an easier test is greater than 144 and the sum of the per cents. at these three ages passing a more difficult test is less than 144, therefore if the sum of the per cents. of children at the ages of 8, 9, and 10 is 144, we know it is a 9-year test. Similarly, if the sum of the per cents. of children at the ages of 8, 9 and 10 passing a test is 161 (36, 55, and 70, see Fig. 11) we should consider the test standard for the age of 8⅓ years.

In order to find the age for which a test should be considered standard when passed by three per cents. at any three consecutive ages of which the sum is *any* number, a diagram (Fig. 13) was constructed. It will be noted that the slanting line labeled 8–9–10 crosses the vertical line 9 at a height opposite 144 on the vertical scale. This indicates that that test is a 9-year test if the sum of the per cents. of children passing it at the ages of 8, 9, and 10 is 144 (29, 50 and 63). To find the age for which a test should be considered standard if the sum of the per cents. of children passing it at the ages of 8, 9 and 10 is 161 (36, 55 and 70) we note

that the slanting line 8–9–10 attains a height equal to 161 in the vertical scale at a point *p*, denoting that the test is standard for the age of 8⅔ years, which corresponds to the age at which 50 per cent. would be expected to pass it, as

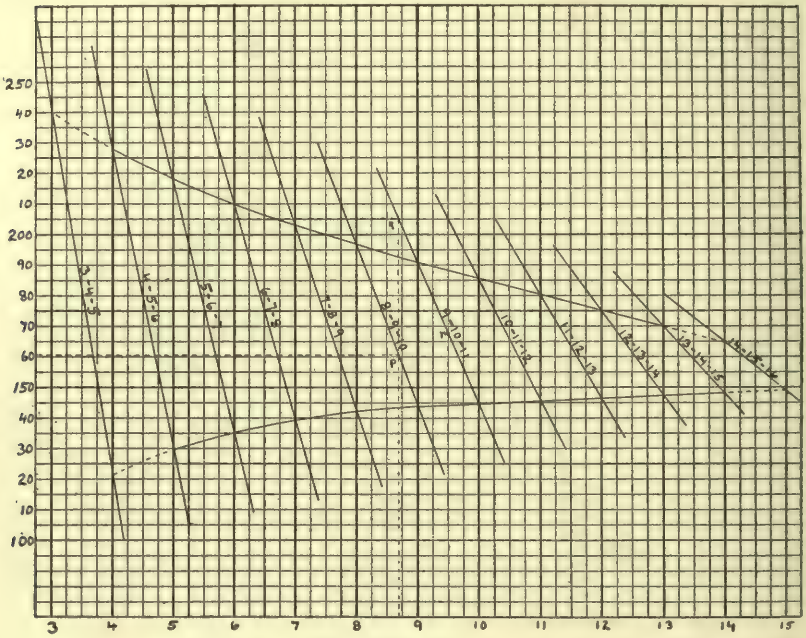

Fig. 13. A diagram for standardizing tests when the sum of the per cents. of children passing it at three consecutive ages is known.

shown by the dotted line in Fig. 11. It will be seen in Fig. 11 that the sum of the per cents. of children of ages 9, 10 and 11 which would pass the same test is 205 (55, 70 and 80). By noting also the point *q* (Fig. 13) at which the slanting line (9–10–11) attains a height of 205, we might have determined the age, 8⅔ years, for which the test is standard.

Returning to the test passed by 44 per cent., 50 per cent. and 74 per cent., respectively, of children of 9, 10 and 11 years, the sum of the per cents. being 168, we may note the point *r* in Fig. 13 at which the slanting line 9–10–11 attains a height of 168 and find that it is opposite a point on the horizontal scale which would be 9½, denoting that the test

should be considered standard for the age of 9½. This is what we found by taking the averages of the ages of standardization found singly from the three per cents. by the former method. We therefore have two methods by which a test may be placed by the consideration of at least three per cents. By the former method, of course, as many per cents. may be utilized as are available, thus making possible in some cases the combining of the results of several different investigators. It was thought best not to attempt a diagram for the sum of more than three per cents. since the data do not seem sufficiently definite in the matter of per cents. passing a test at the ages of two years below and three years above the age for which it was estimated that 50 per cent. would have passed it. Furthermore, as will be shown later, if the three central per cents. do not sufficiently conform to the curves in Fig. 11 the test is not a proper one to use.

Fig. 14 shows the distribution of the tests used by Kuhlmann in obtaining certain per cents. which he has found.[1]

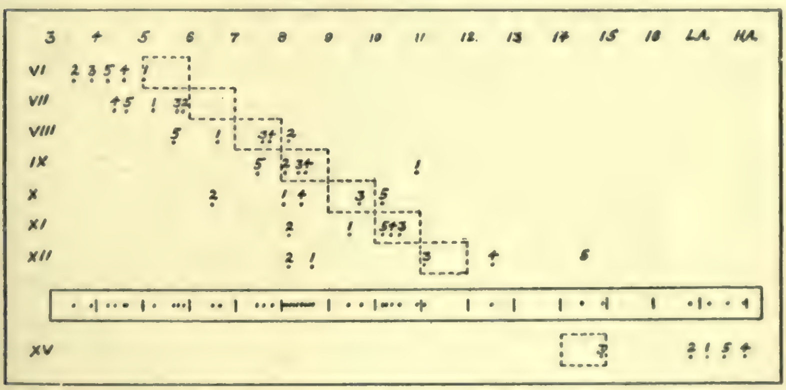

FIG. 14. Showing the distribution of 40 tests according to per cents. of children passing them obtained by Kuhlmann.

These tests were placed by means of the sum of the three per cents. nearest 50 per cent. and the use of the diagram in Fig. 13 wherever possible. In certain cases in which all the per cents. lay above or below 50 per cent., it was necessary

[1] F. Kuhlmann, 'Some Results of Examining a Thousand Public School Children with a Revision of the Binet-Simon Tests of Intelligence by Untrained Examiners,' *J. of Psycho-Asthenics*, June, 1914, p. 255.

to make an estimate by the average method, using Fig. 12. The per cents. in the cases of most of the tests in groups VI. and XV. were so far from 50 per cent. that only a rough estimate could be made. The figures 2, 1, 4, 3, and 5 opposite the number X. at the left, for example, indicate that in group X., test No. 2 was in reality standard for about the age of 6.5 years, test No. 1 for about the age of 8 years, No. 4 for about the age of 8.4 years, No. 3 for about the age of 9.6 years, and No. 5 for about the age of 10.2 years. If the tests are to be scored so that a child passing all the '10-year' tests, for example, and no others, is accorded a mental age of ten years, then those tests must be placed in the 10-year group which are standard for ages averaging 9½ years. Since this is believed to be the method used in deriving mental ages from these tests, they should have been standard for ages falling within the small dotted rectangles. While the placing of the tests could have been done more accurately if more data had been available, it is obvious that many of the tests are far from being properly placed. The distribution of all the tests taken as a scale is shown in the long rectangle.

It will be noted that the overlapping of the distribution of one age group with that of another is partially compensating, so that the scale as a whole is not as far from accurate as the individual groups. In fact, it is not the overlapping at all which vitiates the scale but it is the irregular spacing of the tests. It is, of course, of no great consequence how the tests are grouped for scoring on the testing blank, but it is of great importance to know that they are more accurately spaced on the true scale than is the case in Fig. 14. The presence of too many tests between the ages of 8 and 9 on the scale operates to make a break in the age distributions such as that shown between the 9-year-olds and the 10-year-olds in Fig. 5.[1] In a case of this kind, tests where too thickly grouped should be either altered or replaced by tests which would fill in where sparse. Later will be discussed the need of variability in the spacing of the tests.

(To be continued)

[1] Most of this error has been corrected in the Stanford Revision of the Binet Scale.

COMPLETENESS OF RESPONSE AS AN EXPLANATION PRINCIPLE IN LEARNING

BY JOSEPH PETERSON

University of Minnesota

Though in more or less agreement with the recent discussions on the subject of selection principles in learning by Carr[1] and Watson,[2] the writer is of the opinion that an important principle or consideration is omitted in both discussions. It is to be noted that neither of these writers has made any claim to an attempt at completeness in his suggested explanations. Watson omits the factor of intensity and associates 'the process of substitution' with recency and repetition, though in a manner that hardly makes it coördinate with the latter factors.[3] He also mentions, without making further use of them, certain other factors, all of which are included with substitution in the principle here suggested by the writer. "Unquestionably," Watson says, "the principles of reënforcement, inhibition, and summation of stimuli are constantly operative. When the separate stages of habit have been more carefully analyzed we can more readily see how such factors operate in detail."[4]

Some years ago the writer attempted an explanation (unpublished) of the 'stamping-in-process' of successful acts on the principle of frequency in about the manner suggested by Stevenson Smith,[5] whose article he had not at the time seen. While the frequency factor, or principle, cannot cover every case of learning, and in itself is probably insufficient in any one case, its *modus operandi* has been indicated in a

[1] Carr, H. A., 'Principles of Selection in Animal Learning,' PSYCH. REV., 1914, XXI.

[2] Watson, J. B., 'Behavior: An Introduction to Comparative Psychology,' 1914, Ch. 7.

[3] *Ibid.*, pp. 272 ff.

[4] *Ibid.*, pp. 275, 276.

[5] *Jour. Comp. Neur. and Psych.*, 1908, 18.

general way by Carr and Watson. Professor Meyer has attempted a mechanical statement[1] of what may conceivably go on in the nervous system in the learning act.

The value of the factors of recency and (especially) intensity can be adequately stated, it appears to the writer, only in the case of the joint action of a number of tracts; not in terms of any one single neural tract. There is no question that many of our attempted neural explanations involving one arc, or at best a few neural arcs, are altogether too simple adequately to explain in such physical terms as we desire how one act can survive over the other more or less random acts because of its greater success in meeting the needs of the organism. Even in the simplest act involving so-called conscious control numerous neural tracts are called into play, varying in degree of directness or indirectness of connection with the muscles immediately concerned in the act. Professor Meyer's explanations are suggestive in this regard, but not of much use as they are based on analogies chosen from mechanical structures which make experimental application to behavior phenomena difficult. In a complex condition such as we actually find in the nervous and muscular systems, where various more or less related acts are involved in each reaction, some of these acts may be of an inhibitory nature to others under certain circumstances, while occasionally under other conditions all may tend rather positively to aid or strengthen one another. These mutually inhibiting or reinforcing effects would be determined not only by the nature and complexity of the stimulus but also by the inherited and acquired disposition—neural connections, bodily structure, etc.—of the organism. I shall refer to these mutually reinforcing and mutually inhibiting functions, in all the degrees between these two extremes, as the *principle of completeness of response.*

In certain passages Dr. Carr, in the article referred to, seems to have come close to this principle. *E. g.*: "From the standpoint of the immediate sensori-motor situation in which the animal is placed, the true path and the cul de sacs

[1] Meyer, Max, 'Fundamental Laws of Human Behavior,' 1911.

are to be distinguished from each other on the basis of the degree to which they impede or encourage the animal's activity. A blind alley . . . means hesitation, caution, investigation, or disastrous sensory consequences. The true path presents fewer obstacles; it offers greater encouragement to freedom, continuity, rapidity, and vigor of motor expression.[1] The difference is merely one of *degree*. The blinds check, thwart, and suppress activity more than does the true path, while the latter encourages and facilitates activity more than does a blind alley. The principle of relative intensity is here effective; acts are selected or eliminated according to whether the sensory consequences tend to facilitate and intensify them on the one hand, or to disrupt and suppress them on the other."[2] Yet this suppression or facilitation is here in no way explained by the principle of relative intensity; relative intensity, as well as the suppression and encouragement, of certain of the attempted acts is rather a consequence of a sort of cumulative attitude, or incomplete activity brought about by the overlapping of partially complete responses. Again, "The animal does not react to this complex situation as a unitary whole, as a single stimulus. He reacts to it selectively, and as a series of stimuli. There is a circular interaction between the sensory stimuli and the animal's movements. Each act modifies the stimulus in some respect, and the change of stimulus in turn modifies the act."[3] Here Dr. Carr recognizes the need of the complex situation affecting the response, though the nature of the selection is not made clear.

In the case of the maze problem the animal on entering a cul de sac—or any other path, in fact—responds at first more or less incompletely, *because all the subordinate activities involved cannot take place at once*. If the animal's progress is soon checked in a blind alley the animal is not seriously nonplused. Certain elements of the general response are tending to drain into other alleys that may recently have been passed,

[1] This is of course true not for any momentary status of the animal but only for a larger situation involving successive stages of acts, or series of acts.

[2] *Op. cit.*, p. 162.

[3] *Ibid.*, p. 157.

thus partially dividing the animal's activity. These elements now prevail when the others are checked. Let us suppose that the correct path, *A*, has just been passed when the animal suddenly comes to the end of the cul-de-sac, *B*. The tendencies to respond to *A* are still surviving and now direct the impeded activity into this, the successful, path. If, on the other hand, the correct path had been chosen the first time the distracting impulses toward *B* would have become fainter and fainter as the animal proceeded into *A*, and would finally have faded away. The principle is not different when the complexity of the situation is increased. When the food is finally reached all the remaining delayed reactions, the tendencies, still persisting, to go into other alleys recently passed, are relaxed—the act as a whole is complete.

Thus by an actual overlapping of many tendencies to respond in diverse ways the erroneous tendencies are directed into the successful ones, and the latter are strengthened by reinforcement. Without such overlapping of various impulses in the same general response, the inhibiting effects of the successful upon the unsuccessful or irrelevant tendencies are incomprehensible. How can a successful result act backwards and strengthen the impulses leading up to it and stamp out the unsuccessful impulses? It is a mistake to look upon these tendencies as separate *acts* each complete in itself and occupying the whole arena for the time being. This seems to make clear why the pleasurable act survives over the other acts: the pleasure itself is not a cause or natural antecedent of the surviving act, but only the inner or 'felt' aspect of it and therefore valueless in explanations, though no less a fact to the individual performing the act. If analogies help us in conceiving this selective process we can find very good ones in a stream of water making its way initially over an uneven and loose soil. Now the water plunges mainly into this little hollow place drawing noticeably upon neighboring portions of the stream; now, this place being filled (cul de sac), the principal part of the current passes on to fill some other depression into which a small overflow had

already begun but was impeded by the main plunge of the stream into the preceding hollow.

The selectiveness, then, is due finally to the entire conformation of the organism together with the present more or less complex stimulating conditions; more immediately it is due to the cumulative effect of various incomplete partial-responses. This is admittedly a rather complicated matter to introduce, too complex adequately to state in terms of simple nerve tracts. Yet without considering the whole situation *together* such terms as free or impeded activity can hardly mean anything. The selectiveness of the organism is simply its more easy adaptation to certain direct and indirect stimuli than to others; but worked out in detail this is not a simple matter. In the case of impeded activity there are more internal conflicts—conflicts among elementary neural and muscular processes than in unimpeded activity. The latter type of response is more *complete*, or unitary, than the other. That responses are always more or less complex is a fact that is not fully enough considered in our usual simple neural explanations. And more complex explanations in terms of nerve impulses are extremely difficult, because of insufficient knowledge on many points of importance. The usual statement is that a lessened resistance is formed along certain tracts due to repetition, or recency, or intensity, or to the combined action of any two or all three of them. Intensity (of what?—stimulus or response? or both?) obviously implies on the motor side a harmonious action of a system of tracts and of various muscular responses, a mutual reinforcement; while on the sensory side it may mean a more effective stimulus for such harmonious activity, not merely physical intensity. The latter condition is illustrated in the heightened effect of a very weak stimulus that is intensely interesting, such as a moving object. The interest is obviously due to some bodily organization, inherited (instincts) or acquired (associations), making certain kinds of responses complete and others considerably impeded on account of the inhibitory, or mutually blocking, action of the constituent elements. The pleasurable tone which accompanies certain

of our acts is of course only a subjective indication that the response is along the line of least resistance. This is true only up to a certain limit at which the act approaches a neutrally toned reflex. We are coming to the point now in psychology at which we cannot look upon states of feeling as *causes* of action. The same is true, of course, of 'ideas.'

The neural correlates of learning processes could not be stated in terms of changes along any particular tract even if it were conceivable that certain of the 'controlled acts' involve but a single nerve tract. Such processes really involve more or less complex *attitudes*, and light is thrown upon them by the delayed reaction experiments of Hunter and others. On account of the complexity of stimulating conditions—some stimuli being direct, others indirect by means of association, and all varying enormously in their degrees of intensity—the various elementary movements involved are doubtless always more or less in conflict; *i. e.*, the total reaction is in a degree incomplete, tentative. It is conditioned by various muscular 'sets,' or tensions, partial responses to immediately distracting stimuli, which cannot relax wholly until relief is obtained from confinement, or food is reached; and even then they likely fade away gradually, if we may trust introspection of our own attitudes. If Dr. Watson will permit a bit of anthropomorphism—a fault of which his recent book is not, by the way, wholly spotless!—this hesitant, delayed, or incomplete response is such as a person may experience in relation to the position of the head if he has just passed some mischievous lad preparing to throw a snowball at him. The attitude may disappear rather soon after the ball has whizzed by the ears; it fades slowly if the ball does not come at all until one is out of reach. This incompleteness in reaction need not imply the sort of imagery which many comparative psychologists are desirous of keeping out of explanations in animal behavior. It involves nothing more in this regard than do the delayed reactions which Watson attempts to explain on the basis of continuous bodily orientation, together with minor muscular tensions and neural inhibitions and reinforcements not observable externally.

These neural processes *may* of course in the case of man, and possibly also in the case of some of the higher animals, be accompanied by the consciousness of images. The writer is convinced that his own auditory and visual 'imagery' is mainly, or almost if not quite entirely, a matter of finer muscular adjustments and motor tendencies of body, head, eyes, and certain ear structures. He is not inclined, however, to deny that certain persons may be able to call into play a richer supply of brightness, color, and tone qualities to supplement these muscular tensions though dependent upon them. But, again, *images are not consistently regarded as causes;* they are the inner aspects of certain results of direct or indirect stimulation.

The solving of any problem, such as the inclined plane, involves numerous synchronous more or less subordinate responses, as well as a complexity of stimulating conditions. Some of these responses are certainly much more immediate and direct than are others. There is therefore a continuous overlapping of responses, some of which are in opposition while others are mutually helpful and serve to the main response as additional stimuli, the latter leading to a more easy and complete expression. In our observation of animal behavior we have been too much interested in the principal response of the animal and have neglected to note sufficiently all the subordinate attitudes and responses. This is to be expected in so new a science, one, moreover, in which we are obliged to resort to objective methods. The inadequacy of the usual procedure comes out more conspicuously, it must also be noted, only when the nicer questions of explanation of the learning process arise.

Certain acts on the whole are chosen, to recapitulate in part, not because they are pleasant but because they are on the whole the most natural. A wrong view on this point leads to serious complications in the explanation and control of behavior, and in the field of human conduct it has done much toward making the study of ethics a mere academic affair and in many of its stages and aspects surprisingly unscientific. That the more complete reactions, those finally

'chosen,' are the pleasant ones in the main is simply an indication of a subjective kind that the response is relatively complete, unimpeded, that it is harmonious with one's inherited and acquired organization; pleasantness, however much a fact—and a desirable fact—it may be in our lives, is not a serviceable explanation principle. This fact is made patent by two millenniums of ethical discussion, and comes out more strikingly in the simpler experimentally controlled behavior of lower animals. Since adjustment is hardly ever quite complete or at an end with any particular act, there may with rather short-ranged or temporary successes be momentarily increased activity and therefore a pleasant tone, only to be checked later. Such a procedure is evident even in a more nearly homogeneous mechanical process like a liquid flowing over an uneven surface.[1]

The recognition of this principle of completeness of response, or the overlapping of responses, makes more comprehensible than heretofore results obtained by certain experimenters, showing that an animal which has learned certain problems adapts itself to others—*i. e.*, learns them—more readily than one which is wholly untrained. In the former experiences the animal has habituated itself somewhat to the incomplete or tentative attitude so that it can more readily remedy its errors, or find the right response. Among themselves animals differ widely, as do also persons, in the degree to which they throw themselves whole-heartedly into each possible outlet that presents itself in a difficulty. Experience has important effects in modifying this general aspect of behavior. The general unnaturalness of apparatus to an animal is sufficient readily to give the animal this tentative attitude even though the new problem requires a response considerably different from those on which it has been trained. Of course in cases of this kind various factors enter, all of which are not reducible to any one principle.

In the case of the inclined plane problem it is noticeable

[1] Whatever is ultimately true—if ultimate in this sense has any meaning at all—as to the 'freedom of the will,' scientific explanation seems to be most successful in the field of behavior, as in other fields, when it proceeds on the assumption of determinism. Consider, *e. g.*, certain recent progress in applied psychology and sociology.

that rats proceed to the plane, after failure to get into the food, with considerable tension toward the door of the food box. Frequently at a certain stage in the learning the animal comes to a standstill, and then scurries back in vain to the door. The uncritical would say that the rat was trying to recall which way it went to success before. Since it must invariably push the inclined plane before entrance to the food can be obtained it is forced in every case to return to the plane, but it does this as if an elastic cord were constantly pulling it toward the food. These muscular tensions are released only when the proper reactions have made entrance to the food possible and when the food has been reached. In this way all the relevant acts are associated effectively together by what is tantamount to *simultaneous action.* The apparent backward effect of certain acts is thus made comprehensible. Even in cases when the animal goes directly to the inclined plane, if the habit is not fully established, it frequently shows hesitation and the effects of impulses toward the food box. The writer has frequently noted this hesitant behavior, and Dr. Florence Richardson's monograph supplies many instances.[1]

Applying this principle of overlapping of incomplete responses to a specific instance we find it of considerable help. Take the case of the choice of the correct alley as against the cul de sac discussed by Watson[2] on page 267. The figure is here reproduced. If the animal goes in the direction indicated by the arrow in *A*, it is not simply a matter of probability as to whether it will finally establish the habit of turning into *B* —for this probability argument gets very flimsy when you take into consideration a number of successive culs de sac. As the animal passes *B* on its way to *x* there are impulses to enter *B*, but they are outweighed by those tending

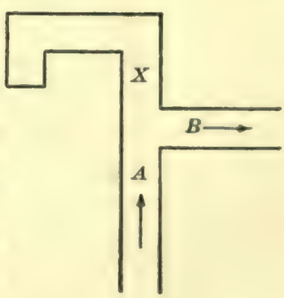

FIG. 1.

[1] 'A Study of Sensory Control in the Rat,' *Psych. Rev. Mon.*, 1909, Whole No. 48.
[2] 'Behavior.'

into X. As it returns from X these impulses, still carried over to an extent and therefore still effective, are potent toward directing it into B. Besides this fact is another one, based on the same principle: the attitude resulting from the general direction of the animal's moving, so long as it has not retraced its steps except in a blind alley, is operating against its returning into A in our figure. A study of illusions usually called being 'turned around,' to be published later, has convinced the writer that we are yet too neglectful of these larger attitudes in our studies of behavior. The correctness in general of this idea of the overlapping of responses in learning as a principle of explanation seems to be supported by the fact that a short cul de sac is less confusing than a long one, even though neither of them has any turns. This is true of very short ones; how far it will hold true in general is a matter yet to be determined, one the working out of which will make an interesting modification of the delayed reaction experiment.

A CASE OF PSEUDO-PROPHESY

BY LILLIEN J. MARTIN[1]

Stanford University, Cal.

At the time of the great earthquake of 1906 the accompanying poster of the Junior Farce PKWTNOPIU of the Class of 1903 which was written by Harry Johnson and Fletcher Wagner, was referred to as a prophesy by some of the papers of San Francisco. As it seemed of interest to ascertain whether this was actually the case I wrote to Mr. Johnson, a consulting geologist in Los Angeles, the maker of the poster, in regard to the matter. His letter which I append shows that the contents of the poster[2] grew out of the drawing of an inference regarding the future from the past—is a case, in short, of scientific prediction.

Mr. Johnson says in his letter of Aug. 11, 1913:

"Mr. Fletcher Wagner, whom you may remember as winner in the Carnot Debate several years ago, first suggested to me the idea of a class play which should be like the famous breakfast food, 'something different.' At that time I was full of the geologic phenomena associated with the earthquake rift which passes southeastward along the San Francisco peninsula, back of Los Gatos, and so toward the Pajaro River. I have seen the effects of crustal movement during past ages along this fault line and have been deeply impressed with the topographic changes which have taken place in this part of California within comparatively recent time (geologically speaking). All of this was, of course, as you know, before the earthquake of 1906, but I realized that the fault line had been the theater of earthquake activities on a grand scale for a tremendous period of time.

[1] Read at the joint meeting of Section H of the American Association for the Advancement of Science and the American Psychological Association, San Francisco, August 3, 4, and 5, 1915.

[2] See the plate which gives a reproduction of the poster itself and of a photograph of Memorial Arch as it appeared just after the earthquake.

"What better than that Mr. Wagner, with his eye for the dramatic, should see in this geologic fairy tale a chance for a play that ought to appeal to at least Dr. Branner's students. Hence we sate ourselves down and began this immortal work. Fletcher composed the music, words, songs and pretty much everything else, so my creative outburst expressed itself, so to speak, volcanically. I knew that the earthquake rift ran northwest and southeast and felt that any high structures near it would probably be toppled over in case of an earthquake. The highest structure at the university, except the chimney, was the arch, and that seemed to lend itself best to a poster. I wanted the poster to be graphic, rather smashing in its effects, if you will, and so pulled out one side of it and left the arch overhanging in an impossible manner (I hope the engineering profession will have forgiven me this by now). Otherwise I tried to show what I thought would really happen in case the fault line had a chill. So far as any premonitions were concerned I know there were none. I merely drew a poster as best I could on the evidence of geologic facts gathered in the field. As I recollect, the poster was discussed considerably at the time of the earthquake in some of the San Francisco newspapers and of course, the usual vivid imagination of our newspaper friends called forth a long tale of the astounding clairvoyance of a Stanford student. Bosh and rot. You now have the real inwardness of this remarkable event put on paper for the first time."

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SOME LOGICAL ASPECTS OF THE BINET SCALE

BY ARTHUR S. OTIS
Stanford University

Part II

The Criteria of a Test of Intelligence

(*Continued from page 152*)

If we consider intelligence as a function of age, we will, of course, expect to find the per cents. of children at succeeding ages who pass a given test to increase in a more or less regular way as indicated by the curves in Fig. 11. If the per cents. at succeeding ages do not increase regularly, as in the case of tests XII., 2 and 5 (Kuhlmann, p. 256) of which the per cents. are plotted in Fig. 9, we must conclude either that the data are too meager or else that the test is not a good test of intelligence. In the tests cited the irregularity is doubtless due largely to smallness of numbers tested. Having sufficient data, however, we would have a partial criterion of a test of intelligence. It is but partial for the reason that if the per cents. of children passing a test at successive years do increase regularly we cannot be sure that it is a test of intelligence, since there are other abilities that are also functions of age. Thus, the per cents. of children at successive ages who could span five feet with the two arms would without doubt also increase regularly, since physical growth is also a function of age. There is probably no causal relation, however, between physical growth and intelligence, barring the factor of age. We are therefore in a position to reject certain tests of which the per cents. of children passing do not increase with suc-

ceeding ages; but we must look for further assurance that a test, of which the per cents. passing at succeeding years do increase regularly, is a test of intelligence.

If 500 of 1,000 ten-year children can span five feet with the two arms and 500 of the same 1,000 have intelligences above standard ten-year intelligence, the first 500 will probably not be at all identical with the second 500. It may be that 250 of those who can span five feet are above, and 250 below ten years in intelligence. Similarly, if we knew that 500 out of 1,000 ten-year-olds passed a certain test but that 250 of these had an intelligence less than ten-year intelligence and 250 of those who failed the test had intelligences above the ten-year level, we would be forced to say that in all probability there was no connection between intelligence and the ability to pass the test, or in other words that it was not a test of intelligence. If, however, 450 of the 500 who passed the test had intelligences above the ten-year level, we would be more inclined to think that the ability to pass the test was related to intelligence, to a certain degree at least, and if the 500 who passed the test were the identical 500 whose intelligences were above the ten-year level, we would be sure that the ability to pass the test was very intimately related to intelligence; in other words, that the test was a very good test of intelligence. Thus, if 50 per cent. of ten-year-olds pass a test, the degree of merit of the test as a test of intelligence is measured by the degree in which those who pass it are identical with those whose intelligence is known to be above ten-year intelligence. This degree can be found by correlation.

It will be seen that in order to compare the individuals passing the test with the rank order of the group in intelligence, it will be necessary to have some measure of intelligence, tentative at least. This we may obtain by assuming first by inspection that certain tests which correlate well with age are good tests of intelligence, then building a scale with these, placing each test on the 50 per cent. basis, then testing the coherence of the scale; that is, seeing how well the results of each test correlate as indicated above with the

intelligence order as established by the tentative scale. Then by the process of refinement, that is, by the casting out of incoherent tests (those the results of which do not correlate well with the others taken as a whole), we may eventually more and more nearly approximate a coherent system of tests.[1]

A better method perhaps would be to set up a criterion of intelligence apart from the tests themselves to which the latter may be compared. Thus Simpson,[2] in a study of

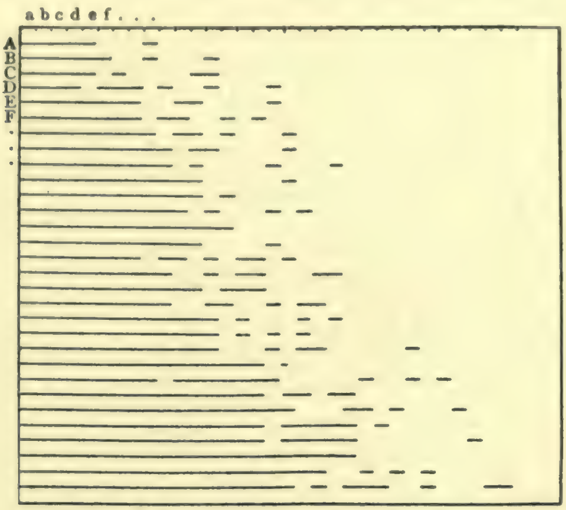

FIG. 15. Showing the distribution of the individuals passing certain tests in a series ranked in order of intelligence on the basis of the whole number of tests passed.

correlations of mental abilities, choses "two extremes of 'general intelligence' as judged by the world." As a 'good group' he chose seventeen professors and advanced students and as a 'poor group,' twenty men 'who had never held any position demanding a high grade of intelligence.' Burt,[3] in a study of the correlation of the specific abilities, had the head master of the school and others rank the boys tested on the basis of general opinion as to their intelligences.

[1] This method has been used by Terman in selecting tests for the Stanford Revision of the Binet Scale.

[2] Benj. R. Simpson, 'Correlation of Mental Abilities,' Col. Univ. Cont. to Educ. no. 53.

[3] Cyril Burt, 'Experimental Tests of General Intelligence' (two figures), *Brit. J. of Psych.*, Dec., '09.

Fig. 15 shows the actual results obtained by Terman in submitting certain tests, *a*, *b*, *c*, *d*, *e*, *f*, etc., to certain individuals *A*, *B*, *C*, *D*, *E*, *F*, etc., of a given age. The presence of a line indicates that the individual passed the test. The individuals have been arranged in the order of their mental ages as computed from these tests and the tests have been arranged in the order of difficulty as determined by the number passing each. While these numbers of tests and individuals are far too small upon which to base a judgment, the figure will serve to illustrate the difference between tests the results of which correlate well with intelligence, as exemplified by test *c*, and those of which the results do not, as exemplified by tests *e* and *f*.

It must be realized that it is entirely possible, theoretically at least, to have a perfectly coherent system of tests which are not tests of intelligence. If, for example, we had begun by choosing tests of physical strength, then by the method of choosing further tests by their degree of coherence, we would be building a coherent system of tests not of intelligence but of physical strength. Therefore it is necessary to use a third criterion in our choice of tests, more particularly at the start when we are choosing those tests with which the later choices are to be compared. In fact, our definition of intelligence is, in a measure, established by the first choices. Suppose, for example, that our first tests chosen were tests of reasoning power; later tests, chosen because of good correlation with the first, might give us quite a different type of tests from what we would get by beginning with memory tests.

It should be noted that whether or not we consider that the degree in which a test is a test of intelligence is measured by the degree of correlation between it and the other tests of the scale, depends upon whether or not we consider intelligence as a 'general factor.'[1] For example, suppose it is suggested to incorporate in the tests a test of the ability to 'carry a tune.' We know that the lack of this ability is

[1] For a discussion of this point see B. Hart and C. Spearman, 'General Ability, Its Existence and Nature,' *Brit. J. of Psych.*, 1912, 5, 51–79, also Burt, *loc. cit.*

largely attributable to lack of endowment of a certain character. If we consider success, adjustment, adaptation, happiness, etc., as aims, and the attainment of these as a measure of intelligence, then we can hardly exclude musical ability from the category of intelligence. But because the ability to 'carry a tune' is not particularly related to the ability to pass the majority of the other tests already chosen to test intelligence, it is considered by some as not a test of intelligence. There would seem to be no reason, however, why the element of endowment in the ability to carry a tune should not be considered as an integral part of intelligence except as we desire, arbitrarily, to define intelligence in another way, for example, as a general factor, or as that character common to the majority of tests. Without going into the discussion of the distinction between the kind of acts requiring intelligence and those which do not, is it not obvious that such a distinction is made, either consciously or unconsciously, when those tests are chosen which are to serve as criteria of those to be chosen because of coherence with them?

To sum up, the three criteria of a test of intelligence are, (1) Do the per cents. of children at succeeding ages who pass the test increase regularly? (2) Do those who pass it also exceed those who do not, in the number of other tests passed? and (3) Are those who pass the tests generally conceded to be more intelligent than those who do not?

The Spacing of Tests on the Scale

As Dr. Terman has pointed out, there is one consideration in the way of placing the tests at equal intervals along a year scale, that is, a scale in which equal units measure successive years, and that has to do with the character of the growth of intelligence. Yearly increments of growth of intelligence are generally believed to decrease with increase of age to about the age 16 or 18. If, for example, the amount of difference between 13- and 15-year intelligence is no greater than that between 12- and 13-year intelligence, then a child would have an opportunity to more than make up for failure of

tests between the ages of 12 and 13 by passing tests between the ages of 13 and 15 on the scale. Therefore, before the scale can be perfectly graduated it will be necessary to know approximately the shape of the curve of growth of intelligence, in order that the tests may be spaced equally as to absolute distances on the scale. If the tests were so placed, the easiest way to score them would be to count the tests passed and from the number found to determine the mental age from a table especially prepared. Thus suppose that from a consideration of the curve of growth of intelligence we deem it advisable to place 9 tests in age-group 3, 9 tests in age-group 4, 8 tests in age-group 5, etc., as shown in Table I., the total number of tests being 88. If these tests were grouped as recommended so that the ten-year group, for example, contains tests which are found by the standardization on the 50 per cent. basis to be standard for ages ranging from 9 to 10 years, then a score of 88 tests correct, or 88 'points,' would mean 18-year intelligence, a score of 87 'points' would mean 17½-year intelligence, a score of 86 'points' would mean a mental age of 17 years, etc., as shown in Table II.

TABLE I

Age-group	Tests
3	9
4	9
5	8
6	8
7	7
8	7
9	6
10	6
11	5
12	5
13	4
14	4
15	3
16	3
17	2
18	2

TABLE II

Tests Passed	Score (Mental Age)
88	18
87	17½
86	17
85	16½
84	16
83	15⅔
82	15⅓
81	15
80	14⅔
79	14⅓
78	14
77	13¾
.	.
.	.
.	.
9	3

It will be seen that in this way the value of a test is automatically determined by the mental age at which the child happens to be. A test in one group is automatically equated to that test in any other group for which it is considered a substitute or the failing of which it is considered as counter-

balancing. In this regard it may be considered a "point scale." A scale of this kind can be made for the degrees of performance in a single function or ability such as immediate memory for digits, etc., for purposes of correlating degrees of certain functions or abilities with intelligence, computed or estimated, etc.

Prognostication

If we knew the exact relation to one another of the curves of growth of intelligence for individuals of varying degrees of brightness, we would be in a position to prognosticate as to the probable maximum intelligence that would be attained

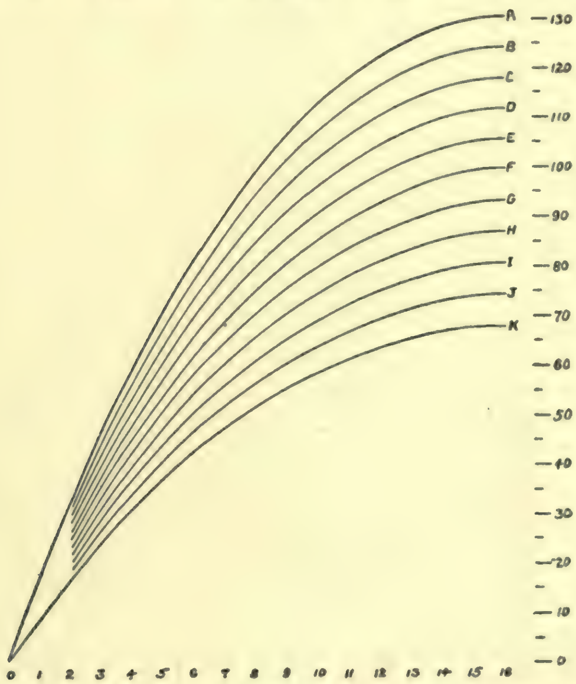

FIG. 16. Hypothetical curves of growth of intelligence of individuals of varying degrees of brightness.

by any individual of whom we knew the intelligence at any age. Hypothetical curves of growth have been drawn in Fig. 16. Curve *F* was drawn first and in accord with the generally accepted theory as to the growth of intelligence.

Then from this curve the other curves were derived on the assumption that all individuals reach maturity at the same age, which in this discussion will be assumed to be 16 years, that they all attain one half of their maximum absolute

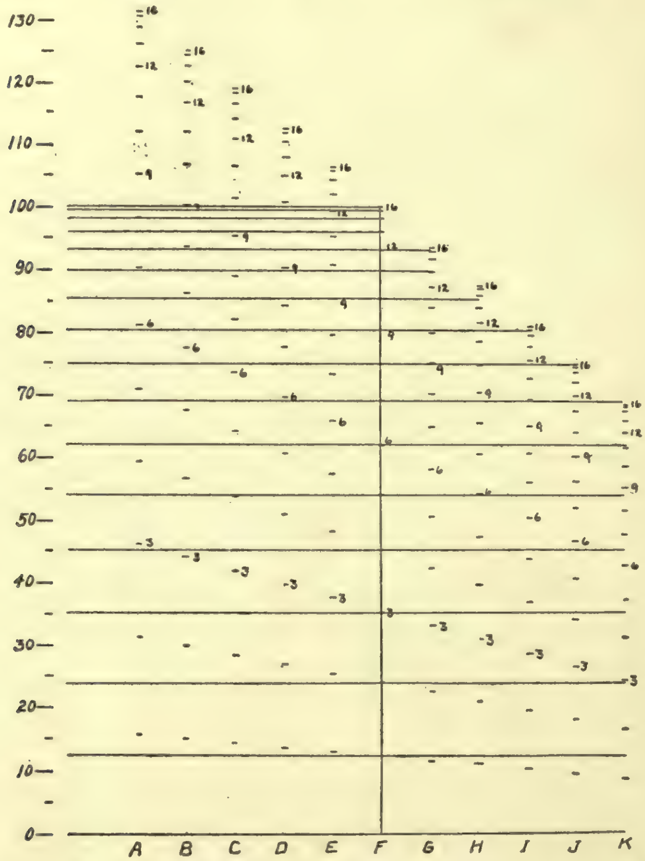

FIG. 17. Showing the relation of the degrees of intelligence at successive ages of individuals of varying degrees of brightness with the degrees of standard intelligence at the various ages and with a graduated absolute scale. This figure was derived from Fig. 14.

intelligence at the same age, and all other corresponding fractional amounts of absolute intelligence at the same age. Guided by this theory, the curves were drawn such that, mathematically speaking, if the equation of the standard curve be considered as $y = f(x)$, in which x measures the

age and y the intelligence, then the equations of all the other curves would be $y = kf(x)$, in which k is a different constant for each curve. Thus since the maximum absolute intelligence attained in curve G was 15/16 of that attained in curve F, the absolute amounts of intelligence or the lengths of the ordinates at all the other years were made just 15/16 of the corresponding ones in curve F, k being in this case 15/16. The other curves were similarly drawn. For the purpose of comparing the absolute amounts of intelligence at the various ages on the various curves Fig. 17 was constructed, in which the horizontal dashes in vertical column A, for example,

TABLE III[1]

Age	Intelligence	Retardation	I. Q.	Intelligence if I. Q. were Constant
6	5	1	.83	5
7	5.8	1.2	.82	5.8
8	6.5	1.5	.81	6.7
9	7.2	1.8	.80	7.5
10	8.0	2.0	.80	8.3
11	8.7	2.3	.79	9.2
12	9.2	2.8	.77	10.0
13	9.7	3.3	.74	10.8
14	10.1	3.9	.72	11.6
15	10.4	4.6	.69	12.5
16	10.5	5.5	.66	13.3

mark the degrees of absolute intelligence attained in successive years on the curve A in Fig. 16. Suppose the mental age of a six-year child were five years and we wish to predict to what maximum intelligence he will probably attain. The method would be as follows, supposing the curves in Fig. 16 to be the true curves. Examining these curves or the columns of dashes in Fig. 17, we would find that the child was progressing along the curve H, on which six-year intelligence just equals

[1] It should be remembered that the figures in this table and Table IV. are taken from a hypothetical curve, the exact shape of which is by no means known. It may be, for example, that intelligence does not reach maturity until the age of twenty, in which case the intelligence quotient would decrease with age at a much slower rate than that indicated in Table III. In fact recent investigation by Terman shows that the intelligence quotient may be considered as practically constant up to the age of fourteen. This fact merely indicates that intelligence really does mature at an age considerably above fourteen. It in no way alters the fact, however, that beyond a certain age, which must be determined empirically, the intelligence quotient cannot be treated as a constant.

standard five-year intelligence. Examining the intelligence of an '*H* child' at 16 years, we find that it is just equal to the standard intelligence of 10½ years, which, therefore, is the probable maximum intelligence that the child will attain. In fact, the probable intelligence at any other age can be found as well. Thus, comparing the positions of the dashes in column *H*, Fig. 17, with the horizontal lines representing the standard intelligence at various ages, we see that the child's intelligence at the various ages would be as shown in columns 1 and 2 of Table III.

The retardation in years is shown in the third column and will be seen to increase with age. This, therefore, cannot be used as an index of the degree of brightness of the child. An attempt has been made to use as an index of the brightness a value called the "*intelligence quotient*" (I. Q.) which is considered to be the quotient of the intelligence at any age divided by the standard intelligence of that age. Up to the age of intellectual maturity this amounts to dividing by the chronological age. The value of this quotient at the various ages is shown in column four in the table. This will be seen to vary also with age although to a less degree than the retardation. In column five are the successive values of the intelligence that we would expect if the intelligence quotient were constant all the way up from the age of six. It will be seen from this column that if we assumed the intelligence quotient constant, we would predict that the child would have an intelligence at the age of 16 equal to standard intelligence for the age of 13.3 years, while by the curves it is found to be equal to standard intelligence for the age of 10½ years. We see therefore that it would not be safe to use the intelligence quotient as an index of the child's brightness, for obviously a child having an intelligence quotient of .83 at the age of six is considerably duller than a child having this intelligence quotient at the age of 16.

When we come to consider the growth of intelligence of a child who is above normal we encounter still more difficulty. Let us suppose a second six-year child is found to have an intelligence slightly above standard seven-year intelligence,

such that he might be considered as progressing on the curve, *D*. (See also *D*, Fig. 17.) In Table IV. are given the mental ages that he would have at the various ages, amount of advance at each age, the I. Q. at each age and the mental age that would be expected if the I. Q. held from the age of six.

TABLE IV

Age	Mental Age	Advance	I. Q.	Mental Age if I. Q. Were Constant
6	7.2	1.2	1.2	7.2
7	8.5	1.5	1.22	8.4
8	9.8	1.8	1.23	9.6
9	11.2	2.2	1.25	10.8
10	13.0	3.0	1.30	12.0
11	16.–	5.0–	1.48	13.2
12	?	?	?	14.4

It will be seen at once that after the age of eleven there is no mental age that we can assign to the child which will be in terms of the intelligence of the standard child. Various terms have been applied to the grades of intelligence above standard 16-year intelligence. They have been called '17-year,' '18-year' intelligences, and 'low adult' and 'high adult' intelligences. Any one of these terms is of course inconsistent with the method now used to designate standard intelligence which for each chronological year is the intelligence of the median individual of that age, for if it be assumed that intelligence reaches a maximum at the age of 16, then '17-year,' '18-year' or 'adult' intelligence is the same as 16-year intelligence except as otherwise arbitrarily defined. While this inconsistency is probably well recognized, a system would, of course, be better in which there was no such inconsistency.

AN ABSOLUTE SCALE OF INTELLIGENCE

If we wish to have a single number as an index of the brightness of any child, we have but to measure the intelligence in absolute units and our index will be the value of k, which is the absolute intelligence quotient. It is the quotient of the absolute intelligence at any age divided by the intelligence which is standard for that age. To find this absolute

intelligence at any age we have but to measure the ordinate of the standard curve at that age. To find the absolute intelligence quotient of a child, for example, who has five-year intelligence at the age of six, we have but to divide the ordinate of the standard curve at five years. For this purpose the absolute scale should be graduated. Let us consider the unit of absolute intelligence to be 1/100 of standard 16-year intelligence. We could then construct a scale, as shown in Figs. 16 and 17, divided into equal units and so graduated that the 100 point measured standard 16-year intelligence. We could then change intelligence of a certain age-grade immediately into intelligence of the corresponding absolute grade. The measure of the absolute grade of intelligence of any child at the age of 16 would be the absolute I. Q. of that child, and vice versa. Now we have a perfectly consistent method of naming intelligences below and above standard 16-year intelligence. Since standard 16-year intelligence is now intelligence of grade 100, above this will merely be grades of 101, 102, etc.

It will be seen that for purposes of prognostication it is not necessary to know the shape of the curves of growth of intelligence. All we need to know is the relation between them. All we need to know is that if the mental age of a six-year child is five years, at the age of seven his mental age will be 5.8 years, at eight, 6.5 years, etc., as given in Table III., column 2. From this table, therefore, we could prognosticate with reference to this child and with other tables with reference to other children without knowing the shape of the curves of growth at all, although of course we could only predict the probable maximum intelligence of a child in terms of the intelligence of the standard child at a certain age and would not know what fractional part this was of maximum standard intelligence.

This brings up the question of the standardization of tests for grades of intelligence above standard 16-year intelligence. This may be done in the following manner. If we assume the curves of distribution of absolute intelligence to be symmetrical this will mean that the grade of intelligence

passed by 25 per cent. of 16-year-olds will be as far above standard 16-year intelligence as that grade is below it which is passed by 75 per cent of 16-year-olds, and the same for the other per cents. Thus if it were found that intelligences graded 90, 80, 70, and 60 were found to be exceeded by respectively 72 per cent., 88 per cent., 95 per cent., and 98 per cent. of 16-year-olds, then we should define those grades of intelligence as 110, 120, 130, and 140 which were passed by respectively 28 per cent., 12 per cent., 5 per cent., and 2 per cent. of 16-year-olds. The per cents. of 15-year-olds, 14-year-olds, etc., which exceeded these amounts could then be combined with these for the purpose of standardizing tests on the basis of several per cents. In the light of the preceding paragraph it will be seen that whether the grades of intelligence called 110, 120, etc., are correctly defined or not will have no particular effect upon prognostication. Whether or not a certain individual has an intelligence just 13/10 of intelligence of 100 (standard 16-year intelligence) is not of so much interest as to know that he has an intelligence exceeded by only 5 per cent. of persons.

The Spacing of Tests on a Scale of Difficulty.—As has been said, we should endeavor to space our tests as evenly as possible on the absolute scale of difficulty in order that a person tested may have the same number of chances to pass a test above the level of his age as he has to fail one below the level of his age. The simplest way to accomplish this is to place an equal number of tests in each space of ten units on the absolute scale, and in order that the scoring might be done with greatest ease this number should of course be ten, preferably, of course, one within each unit. In counting the tests we should then proceed as if the tests extended to the bottom of the scale; that is, a child passing all the tests up to the mark of 50 would be given a score of 50, plus one for every test passed above this point. A person passing tests up to the level of 16-year intelligence would then receive a score of 100. The score of any person who had reached maturity in intelligence would then be a direct measure of his absolute intelligence quotient—the index of his brightness.

The index or absolute I. Q. of a child of a lower age would, of course, be obtained by dividing the score by the absolute measure of the intelligence which was standard for his age. The absolute values, 30, 40, etc., to 100, are seen by comparison of the two scales in Fig. 17 to be equal respectively to intelligences standard for the ages of 2.6, 3.5, 4.5, 5.7, 7.1, 8.9, 11.0 and 16 years. Between each two of these values there should be placed ten tests. The ten tests which should compose the next group should be passed by per cents. of 16-year-olds between 50 per cent. and 28 per cent., the next ten between 28 per cent. and 12 per cent., etc., according to the plan outlined.

Summary

1. Since, for purposes of determining intelligence in age-gradations, standard intelligence for any age has been generally though implicitly defined as median intelligence at that age, from which it follows that the age-gradation of any given amount or degree of intelligence is the age at which it is exceeded by just 50 per cent. of children, and since it is to be assumed that the age-gradation applied to a test is the age-gradation of the amount of intelligence which is required to pass the test, therefore the age-gradation of any test can be none other than the age at which just 50 per cent. pass it.

2. If tests are grouped together in age groups such, for example, that the tests in age group X. are standard for ages ranging from 9½ to 10½, or such that the number of tests standard for ages above ten is probably equal to the number standard for ages below ten (as would be the case by chance if the tests were placed roughly as near ten as possible), or if the tests were all standard for exactly the age of ten, then the proper way to score them would be to give the child tested a score of ten years in mental age if the number of tests passed equalled the number to and including one half of the ten-year tests, and the same for the other years. If, however, the tests are to be scored as at present, giving a score of ten years of mental age only when all the tests to

and including those in group X., or an equivalent number, are passed, then group X. must be composed of tests standard for ages ranging from nine to ten, or all standard for nine and a half.

3. A test may be standardized by reference to the per cents. of children passing it at several different ages or to the per cents. passing it at the same or different ages, obtained by different investigators. In this way greater accuracy may be obtained than by the usual method.

4. To be sure that a test is a test of intelligence, it must be shown, first, that the per cents. of children passing it at the various ages increase with age, second, that the children who pass it are intellectually above those who do not, as determined by a group of tests, and third, that the results of the tests as a whole correlate well with general opinion as to what constitutes intelligence or an established definition of intelligence.

5. If it be assumed that the yearly increments of the growth of intelligence decrease with age, then in order that a child may have no more opportunity to pass tests above the level of his age than he fails below it, it is necessary to put fewer tests in the upper age-groups. This grouping will have to be made at present on the basis of a hypothetical curve of growth, equal numbers of tests being placed in equal portions of an absolute scale derived from the hypothetical curve of growth. At such time as a curve of growth can be obtained from actual data the grouping can, of course, be corrected.

6. If we assume that all individuals attain intellectual maturity at approximately the same age, and that of the final absolute intelligence the relative portion attained by any child at any age is the same as that attained by any other child at the same age, then an I. Q. which is the quotient of the intelligence of the child at any age divided by the standard intelligence of that age (both in absolute units), will be constant for that child throughout the growth of intelligence, and will serve as a fixed measure of the degree of brightness of the child.

CONCERNING THE IMAGE[1]

BY HERBERT SIDNEY LANGFELD

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A few years ago Professor Winch introduced the subject of imagery with a fable about foxes, some of whom had tails and some of whom had not.[2] It was soon evident that those who had been deprived of their brush were meant to symbolize the happy possessors of an imageless mechanism. Considering the manner in which the image has been hunted down recently the simile is more apt perhaps than he realized at the time. Notwithstanding the attack which has been made upon the image I am going to assume that it still exists and shall devote my attention to describing what I believe to be some of its characteristic functioning, hoping that such a course will do more eventually to convince its enemies of its existence than theoretical discussion or introspective accounts of its texture.

In using the term image I do not wish to denote a content of consciousness qualitively different from sensation. Recent experimental work seems sufficiently convincing upon that point. I am referring under that name to sensations centrally aroused.

The meaning of an object is our attitude toward that object, our reaction to it. In like manner the meaning of an image is that inevitable change in our organism which follows its appearance in consciousness. This change may consist of immediate motor discharge and accompanying kinæsthetic sensations or in visceral, glandular or similar reactions characteristic of emotional states. In terms of the self we may say that we assume a definite attitude toward the image

[1] This paper was read at the meeting of the American Psychological Association, on December 28, 1915.

[2] 'The Function of Images,' *J. of Phil.*, 1908, 5, 337–352.

just as we do toward the object. The image acts for or represents the object and in doing so acts as a cue for a definite reaction belonging to the object. Of all possible purposes of an image certainly that of representing a situation not present to the senses is the most obvious.[1]

This function of representation is not, however, characteristic alone of imagery, but belongs also to sensations aroused by an external stimulus, and cannot therefore be used to distinguish it from the latter. The following illustration makes this point evident. If one comes suddenly upon a white sheet hanging upon a line in the moonlight one will probably inhibit one's mechanism for advancing and at least prepare for retreat. This will be readily recognized as a ghost reaction. The situation means ghost. It may be that our sensations will be supplemented by imagery and that we shall actually perceive a ghost. I am inclined to believe that in many instances if not always the imagery which completes the perception accompanies or follows the reaction rather than precedes it and that the externally produced sensation is the sole cue. When we think ghost it may happen that we shall have not a complete image of a ghost but a fragmentary one of the white sheet. Here the image alone is the cue and as in the case of the perception the reaction, to a large extent emotional, is the ghost meaning.

I have purposely chosen as an illustration an incomplete representation, *i. e.*, the white sheet for the whole ghost. The image may be an almost perfect likeness of an object. In most cases, however, it is not; and often it is so far removed from the original object that to say that it means the object

[1] Colvin says in 'The Learning Process,' p. 111: ". . . the meaning of a situation is after all an attitude, and that attitude must in the last analysis be a motor affair. Thus it is but a step to the conclusion that this general dependence of experience for its significance on motor adjustments had left a deposit or 'mind-stuff' that symbolically represents concrete situations, not actually present, but ideally represented."

Hollingworth writes: "Their place (objects of perception) is taken by any content, revived, perseverative, or immediately sensory in character, which happens to be easily available at the time. These processes, by a sort of substitutional rôle, come to represent and impersonate the object between which the relations, as socially recognized, would be said to hold" ('Vicarious Functioning of Irrelevant Imagery,' *J. of Phil.*, 1911, 8, 691).

seems on the face of it almost absurd.[1] But from what we know of the laws of association one thing is certain, namely that a hundred different images can introduce almost the same reaction, and if the reaction is the meaning then a hundred different images can in this sense stand for the same meaning, although ninety-nine of them must of necessity be what I should like to call inadequate images, if I may borrow an adjective used in the case of stimuli arousing paradoxical sensations.

Several years ago I obtained introspective accounts of imagery occurring in experiments on inhibition and hitherto unpublished.[2] As the introspection seems to me especially interesting among other things for the clear evidence of the motor attitude which attaches itself to what without due consideration of this setting would appear totally irrelevant imagery, I should like to introduce it here.

A method of experimentation was desired by which the subject would be required to suppress some very strong association. With that in mind the alphabet and the numerical series were selected. There were five subjects, all students of the Harvard Psychological Laboratory. Each was asked to repeat the alphabet as rapidly as possible and the time was taken with a stopwatch. Then he was asked to repeat the alphabet again omitting the letters f, l, r and x. The signal to begin was given about two seconds after the instructions and the time to repeat the alphabet again taken. This test was given in connection with other experiments, so that only one test was made each week, the series extending through about two months. The next week the subject was

[1] I am thinking particularly of the classic example of the image for 'but' described by Professor Titchener. The presence of inadequate imagery has been repeatedly mentioned in the literature. In the *Philosophical Review*, 1897, p. 650, J. R. Angell writes: "They (images) often shift, and are by no means the same at different times. Indeed, there is no necessity that they should be, representing as they do simply a medium for the performance of a certain function, *i. e.*, the function of meaning a definite thing. Thus the image which serves me in using the concept 'justice' is sometimes a visual image of a state of justice and sometimes simply the auditory word image." ('Thought and Imagery.')

[2] A brief account is given in the report of the meeting of Experimental Psychologists at Clark University, PSYCHOL. BULL., 1912, 9, 236.

asked to omit g, m, s, and y. Similar arrangements were tried for several weeks. Then he was asked to repeat the numbers up to thirty omitting 5, 10, 15, 20, and 25. Then omitting 4, 9, 16, 19 and 26 and so on. Toward the end he was asked to omit the even numbers and finally to repeat the numbers in reverse order omitting 27, 18, 12, 9 and 4. Introspection upon the fore period (V.P.) and the main period (M.P.) was asked for at the end of each test.

From the full introspection I have selected the portion essential to an understanding of the imagery and manner of inhibition of each subject. Arranged according to the subject it is as follows:

Subject *A*

1. V.P. Auditory kinæsthetic, 'Omit f, l, r, x.' M.P. When I came to the letter to be omitted I made a short pause. Visual image of 'f.' Felt an inhibition.

2. V.P. There was a visual image of the letters and auditory image that I must not say them.

3. (As the experiment progressed we find) the fact that I was to omit the letters was not strongly represented in consciousness, but the letters themselves were much more prominent (in the V.P.).

4. V.P. Before I started I went through (skipped) from 5 to 7. 6 came up visually. (He was to omit 6, 11, 17, etc.) There was no tendency to say it.

5. (Finally we have): M.P. When I came to the one to be omitted, it came up as visual and auditory image, but no kinæsthesis. The inhibition was so conscious (powerful) that there was no attempt to say the number to be omitted.

Subject *B*

1. V.P. There was the visual imagery of the number scheme. I thought that when I came to them I must be careful. M.P. When I came to 4 I knew that I must stop.

2. V.P. I suppressed the numbers as I went along. I saw where the letters were. M.P. When I came to the letter before the one to be omitted I got excited. The letter came kinæsthetically. I felt as if I drew back.

3. V.P. There was the feeling of location of the numbers in a scheme. I felt keyed up when I thought of them. There was mild fear when I saw them.

4. V.P. Visual image of the numbers. I thought of the numbers before the one to be omitted as the ones to stop at. Feeling in throat and tongue. M.P. When I came to the one before the letter to be omitted I recognized the feeling and stopped.

5. V.P. I made myself see the number preceding each one. I tightened up my mouth and shoulders. Rigid feeling.

Subject *C*

1. V.P. I tried to think where the break was. I saw the letters as a series of steps with the letters to be omitted cut out. M.P. As I got to the letter there was a visual image which I had to throw off.

2. V.P. I thought of the letters as holes or notches that had to be jumped over. M.P. They came up kinæsthetically. As they came up I saw the breaks. This shut off the letters.

3. V.P. The letters were represented by hurdles to be leaped over.

4. M.P. Hurdles repeated. I did not see the numbers at all. They came up kinæsthetically and I had to inhibit them. All the time I was conscious of the field of hurdles.

5. V.P. The series was represented by a broken chain. M.P. I saw the break when I started. The number came kinæsthetically. I forced it back according to the visual image. That is what seemed to help me inhibit the number. Later the inhibition took care of itself.

Subject *D*

1. V.P. Visual image of immense gaps to be leaped. M.P. When I reached the letters to be omitted there was an auditory image of the letter and a clenching and stopping. A feeling went down as far as the diaphragm.

2. V.P. Visual image of the five letters to be omitted on a card. I knew I should have to stop at f. (g was to be omitted.) Nothing for the other letters. I trusted to luck for them.

3. V.P. Visual image of a stony field. The stones were numbered with the letters to be omitted. They were as obstacles. M.P. The numbers did not come into consciousness. I just stopped for some reason at the number before, and groped about.

4. V.P. Visual image of a football game. The numbers were represented by men whom I was to pass. M.P. I ran with the ball. The man who reminded me of six threw himself forward. I did not say six.

5. V.P. A visual image of a piano key-board. My idea to omit the numbers was to leave out the black keys while playing.

Subject *E*

1. V.P. I went over 5, 10, 15, 20, 25 to myself. I noted that they were at certain stages. I went from 4 to 6. Instruction in mind. M.P. I grouped them in fives, getting the first group from the V.P. As I went on it became easier and easier.

2. V.P. I repeated the numbers five or six times. I do not know how the instruction was present, but I knew that they were not to be said. After memorizing them I was conscious of the instruction to omit them. When I came to 3 (4 was to be omitted) I stopped as if I could not tell what to do. The instruction came into consciousness and I went to 5.

3. V.P. I repeated the numbers to myself and put my fingers down as I said them. I did not think what had to be done to them.

4. V.P. I thought of the numbers preceding the ones to be omitted. When I thought of 19 (18 was to be omitted into the backward series) the instruction was in mind. M.P. When I came to 28 the number to be omitted (27) was in mind, until I started to say 28. It then dropped out and I went over to 26, without 27 coming up again. At about 21 number 18 (the one to be omitted) came up and remained until I began to say 19.[1]

[1] For the most part the introspection, at times inexact in expression, is in the form in which it was given to the examiner. Some of the abbreviated sentences have been completed by me. The subject was not pressed for a more detailed description when he said he 'thought' or such and such was in mind. The experimenter was not at the time interested in the question of imageless thought.

It will be seen that visual imagery was used by four of the subjects and probably by the fifth. In general the method of inhibition was to associate in the V.P. some form of inhibition with the numbers either visually or verbally represented. Sometimes there was a direct association of the inhibition of the vocal organs with the numbers. Sometimes there was an association of the words of the instruction as intermediary. In one instance the fingers were pressed down as the numbers were spoken in the V.P. In another an emotion of fear was associated with the letters. The imagery at times was fantastic, such as notches or hurdles to be passed over or football players to be evaded, but inadequate as they may be they were in all instances of such a nature as to be readily associated with inhibitory processes. They mean 'inhibition' in that they function as such. It will be readily admitted that the perception of the same word can give rise to different meanings according to the setting. In like manner a visual image such as that of a hurdle, which was most strongly associated in the subject's mind with evasion, can in the above situation cause an evasion of a word, that is the inhibition of the vocal organs rather than the more direct association of the lifting of the feet.

In the main period the visual or auditory imagery frequently appeared as a cue and the inhibitory process followed. At times there was probably a direct effect of inhibition from one muscle group to the other. We know that such radiation of motor impulses occurs. The clenching of the fists and the pressing down of the fingers when other muscles are to be inhibited are examples. I feel rather convinced that in the case of inadequate imagery also there is such an action from one muscle center to another, the imagery directly initiating the motor response most readily aroused.[1]

[1] In this connection Wilfrid Lay's remarks ('Mental Imagery,' *Psychol. Monog.*, 1897–99, 2 (3), p. 29) are of interest: "If we attempt to visualize such lines as

'To take up arms against a sea of troubles
And by opposing, end them,'

the result is often the purest visual nonsense." It would seem from what I have said above that the imagery would probably not be nonsense, but a definite motor setting, a brace against an onslaught such as is necessary against the breakers.

The full introspection shows that there is a dropping off of imagery as the tests progressed and even in a single trial the imagery appears only for the first letter or number. The determining tendency—whatever that may eventually turn out to be—grows stronger and reaction follows reaction without the intervention of imagery.[1]

When there was difficulty the image tended to appear again. A good example is that from Subject *B*'s introspection, at the end of the series. He decided not to think of the numbers in the V.P. All went well for three numbers. He failed, however, to omit the fourth and the visual image of the fifth promptly appeared. This reappearance of imagery to help in time of trouble has frequently been observed in investigations on the learning process.[2]

It is undoubtedly because the task set in the above experiments was a difficult one that so much imagery appears so clearly described in the introspection and for that reason the test recommends itself for class-room practice.

The reactions in the above experiments are comparatively simple ones. There is merely an inhibition of the muscles of speech involved. It may be stretching the term to speak of an inhibition as a reaction, but there will be no confusion if

[1] See J. R. Angell, 'Methods for the Determination of Mental Imagery,' *Psychol. Monog.* 13, (1), p. 70 for a good description of the decrease in visual imagery with practice.

[2] D. F. MacLennan writes ('The Image and the Idea,' PSYCHOL. REV., 1902, p. 74): "Another proof of the close dependence of meaning upon imagery is found in the constant resort to imagery when thought is baffled. . . . The moment, however, that some new thought or some new combination of thoughts arises, we search for the concrete imagery in which the conception may be appropriately embodied."

M. R. Fernald ('The Diagnosis of Mental Imagery,' *Psychol. Monog.* 14 (1), p. 137) writes: "That we have had imagery reported for each of the tasks by each of the subjects may be seen by reference to our experimental data. The certainty and invariability of this response increase with the novelty or difficulty of the problem."

Similarly Book says in a footnote ('The Psychology of Skill,' p. 42): "It is psychologically important that all the images which appeared in the different stages of the writing were first prominent and distinct, then hazy and indistinct, disappearing entirely soon after they had served their purpose in the learning."

See also G. H. Betts, 'The Distribution and Functions of Mental Imagery,' *Teachers College, Columbia University Contributions to Education*, 1909, p. 94. He finds that imagery appears when thought is baffled but thinks that it is "for the most part irrelevant, and hence of no possible service in reaching the solution."

we speak of it as a motor setting. In the more intricate meanings we must look for a more highly organized response and this we find in the integrated action about which recent physiology has told us much.[1] And further there is no reason why a fragment of a most simple image may not be the cue for a very complex and intricate response.[2] A mere ghost of an image may serve as a cue for an extremely involved ghost response. This is particularly true of concepts, for a detailed image would have the tendency to inaugurate a dated and placed response, which is contrary to the purpose of concepts. This has seemed to me the explanation of the fact sometimes noticed that imagery of concepts is likely to differ somewhat from reality. I remember in experiments I made a few years ago they were invariably larger than reality; but that is only one of many possible forms of variation.[3]

A type of reaction to imagery which should be more generally emphasized in the literature is the emotional reaction. We found in the above introspection that the emotion of fear was directly attached to the image. This, of course, frequently occurs and leads most readily to a motor response. There is, however, a large field where the motor response as a practical attitude is absent by reason of the very nature of the attitude. I refer to the field of esthetics. Here imagery is very prominent, especially visual imagery, and the response is generally a subtle emotion with its accompanying bodily changes and empathic motor setting. The prominence of imagery in esthetic enjoyment where there is this non-practical attitude suggests a rather obvious explanation why in general comparatively little imagery is noticed and also why, when introspection discloses an army of clear and vivid visual impressions, many of us doubt the reliability of the introspection. The greater part of our attitudes toward our in-

[1] See E. B. Holt, 'Response and Cognition,' *J. of Phil.*, 1915, 12, 399–400.

[2] J. R. Angell (*loc. cit.*, p. 651): "In so far as an image is simply a symbol of certain experiences, great dimness in it is still compatible with its correct use within limits."

[3] In regard to the function of the image as cue and the importance of the motor setting in general ideas see Münsterberg's 'Psychology General and Applied,' p. 173. E. B. Titchener, however, states: "I doubt whether particularity or abstractness of meaning has anything essentially to do with the degree of definiteness of my images." 'Experimental Psychology of the Thought Processes,' p. 16.

vironment are practical attitudes. Our interests are directed toward what we can do with an object and how we respond to it.[1] The objects themselves, from the fact that they are mere cues, remain for the most part in the margin of attention while the reactions occupy the fovea. It is the dreamer, the non-practical man, the man out of touch with his surroundings, for whom visions occupy the center of attention. This difference in attention is greater even when not in the presence of the object, that is when imagery is functioning in its stead. Then the slight imagery present in the extreme margin of consciousness of the trained mind is often as little noticed as the *mouches volantes* before the eyes.[2] To ask a subject to introspect upon his thought process must almost inevitably distort to some degree the picture, by bringing the imagery into too much prominence. Yet an argument against its too great prominence is not one against its existence.

I have spoken of the consciousness of the mature mind. From what we have said of the use of imagery in learning, in acquiring specific adjustments, it would follow that imagery is more prominent in childhood and this is what is generally found. I venture the suggestion that there is also much imagery in old age, but for quite another reason. In the late period of life action is of little importance and contemplation fills the soul. Witness the delight in minute and often tiresome description of trivial details.

In conclusion I would say that I thoroughly agree with the Behaviorists that our efforts should be directed with all possible energy toward the refinement of the methods for the investigation of expression. The science owes them the deepest gratitude for their reaction against objectively uncontrolled experimentation. As a psychologist, however, I cannot be satisfied with half the picture nor can I persuade myself that images which I have experienced do not exist, although they, together with sensations, may eventually be further analysed.

[1] See Judd's 'Psychology,' p. 238.

[2] In the classic words of Galton ('Human Faculty,' p. 112), "That it must afford immense help in some professions stands to reason, but in ordinary social life the possession of a high visualizing power, as of a high verbal memory, may pass quite unobserved."

MOVEMENT, CENESTHESIA, AND THE MIND[1]

BY GEORGE VAN NESS DEARBORN

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Although especially favored by the warning of examples given it more directly than usual by the history of philosophy, it can scarcely be gainsaid that psychology, the most properly basal of all the sciences, has suffered not a little stagnation from an excess of deduction in method; there is induction certainly in quantities to be proud of, but, it seems, from a relatively unproductive source. Psychology in its descriptive phases, and in the abnormal aspects as well as the normal, has most often not been wisely based, not founded "flat on the nether springs" of universal bodily movement and function.

Although philosophically born, so to say, in a psychological laboratory, I believe most thoroughly today that the actual laboratory experimentation, indispensable as it is, is not the directest road to a working and real knowledge of mind as a whole. Much of the practical work, to be sure, has been highly productive, the qualitative research especially. It seems, however, obvious today that the same extent of time devoted to a study of the sensation-basis of mind as inclusive of consciousness but much more extensive (in a figurative sense); dedicated to direct empirical knowledge closely correlated with the neuro-musculo-glandular machinery, particulary the coördinating nervous system, would have taken us further. It would perhaps have already placed psychology preëminent in human wisdom, *the acknowledged science of how to live.* Quantitative measurement (unfortunately inherited, just as the laboratories were opening, from the beginnings of the new physiology), psychophysics, "turned its head" in the laboratories,—toward itself; and naturally the science rejoiced in its escape from phrenology,

[1] Read before the American Psychological Association at Chicago University, 28 December, 1915.

already seen to be lost on the wrong road. Even introspection, direct or indirect, the most natural and fundamental of all psychologic methods, has not always worked in the right "places" in the mind, even if it has perchance labored in the right way. But this is changing.

Introspection on a new basis and by a new method is coming in. Only now, instead of "looking within" and noting what is actually "seen," one "looks within" the total personality and judges as to what he ought to see in the light of advancing analysis of the body and of its meanings in this total personality. We might term this potential or even casuistric introspection:—what might be or ought to be found in this manner—under certain conditions. And such a procedure is without taint of "deduction" and is a perfectly proper and valid psychologic method. Any doubt that some may for a moment have about this, must be ascribed to the relative dimness and indistinctness of the subconscious and what therein is; and yet you all, everyone, would readily admit that a misplaced chair against which you fall in your study is no whit less real because at the moment the study chances to be dark. It is so in the subconscious phase of mind.

Some psychologists still lack adequate knowledge of the body, of the material which composes it, and of the universal *motion* to which, even by the often materialistic physicist, this "material" is ultimately reduced. This latter concept motion is indeed the key to the situation, and the obvious psychologic failure to explain mind (for dualism no less than for monism) seems largely dependent on the presumptious and dogmatic refusal of many to admit this category, spatial dislocation, motion, into their explanations and almost into their psychology at all. These even seem to ignore the fact that now, all the while and everywhere, the conceptual bounds between mind and energy, before assumed impassable, are felt to disappear like fog as we advance into the clear daylight of understanding.

The central truth that seems to me indispensable to present psychologic breadth is that the *organism is in universal move-*

ment, movement continuous in both time and space. This is the category "Life." No part of the action-system while alive ever for a moment is at rest, although often, of course, relatively at rest. This universal movement of the body, entirely "immanent," inherent in the deepest nature of organism, must mean something in the personality; and it must be represented in the concomitant mental aspect of the individual; for human personality, above all other finite integrations or Kantian "unities," is the most complete.

Any physiologist whatever who for a moment tried to make this universality of movement explicit to himself or to another would do so quickly and positively. He would think as the most striking circumstance, perhaps, of the *universal tonus* wholly characteristic of living muscle which is half of the body's mass and nearly everywhere throughout its volume. He would find several degrees of tonus ranging from catalepsy through cramp, exertion, waking activity, lassitude, sleep, coma, and paralysis to death itself, where at last its zero is reached. Muscular tonus is continual during life and well-nigh spatially general, and stands in psychology inevitably for the movement or stress or strain which is the concomitant of afferent neurility.

The *necessity of maintaining equilibrium* as a sensational bench-mark or base of measurement and calculation is another universal need in the organism which in a manner quite similar demands the continuous activity of a body animated everywhere and at all times with a motor fabric. The reader may readily assure himself of this with a little careful consideration of ten actual minutes of his own behavior when about his ordinary activities.

Mechanical purchase for many efforts has to be secured; this one may typify by pushing against the wall and noting the general hardness of the muscles as he does so. This maintenance of a resistance-basis is similar in kind with the preservation of equilibrium; these two plainly are special functions of the universal tonus of the organism.

Sherrington has demonstrated the fundamental principle of neuromuscular action termed the *reciprocal innervation of*

functional antagonists. The principle is self-explanatory and means just what it implies: when the fore-arm supinators, for example, are actuated the pronators are actively inhibited, when the flexors are actuated the extensors are inhibited, and so all over the body. So far as protoplasmic changes in the mechanic stresses and strains are concerned, actuators of the sensation-fabric, this duplex process means a more than doubling of the bodily movement.

In conditions of feeling and of dynamogeny, and proportionally thereto, there is apt to be *irradiation of neurility* in the spinal grey and perhaps in the cortex, "across country" so to say, and not by the more usual channels of communication. One sees this in convulsions, in "hysterics" (and very possibly in delirium tremens). It means a large increase in the motivity of the body.

In addition to this elaborate mechanical vivacity throughout the body's volume, there are the two unending series of specially innervated movements in two classes, vegetative and personal or voluntary, as I have emphasized so much for the comprehension of sundry conditions. The vegetative motions are automatic, reflex, autonomic, instinctive, emotional, or habitual, but each extensive spatially more or less in the body, and each a movement registered by receptors. The voluntary movements I have discussed elsewhere,[1] and they are characteristically universal in the organism both muscularly and neurally. Moreover, there is absolutely no assignable limit to the extension of voluntary control in the vegetative organism.

Finally, mechanical respiration, circulation, digestion, and excretion each are all-pervasive muscular hydraulic processes whose direct influence is continually felt in a material way throughout the organism.

Now pardon me for expressing the firm belief that the only reason the average academic psychologist ordinarily ignores all this mass of intelligent and beautifully adapted movement throughout every human body from birth to

[1] Notes on the Neurology of Voluntary Movement, *Medical Record*, New York, 18 May, 1912.

death, is that he does not *realize* it, does not, practically, in frank truth, know it nor appreciate its meaning in the mental control of behavior; in short its primary necessity to a successful psychology. Some do not realize how most intimately interwoven are automatic movements with the voluntary, or how very readily subsensory impulses become plainly sensory. Plasticity, mental as well as protoplasmic, requires present emphasis.

For years in my psychological classes, for example, we have discussed at some length a topic which would surely meet with the full approval of the great John Locke, wise prophet of several important things whose fullness is yet to come, three centuries after the brain that "synthesized" them was already mould. This topic we termed *Cenesthesia*, explained as good modern American from the ancient "koinos," common, and "aisthesis," sensation. Cenesthesia was said to consist of two complementary groups of sense-experiences: one kinesthesia, particular sensation of movement; and the other the cenesthesia proper,—both together making up the sensation-fabric, the empiric skeleton of mind conventionalized as the subject of a natural experimental science. The kinesthesia is described as including "touch" and with nine or ten functions. These kinesthetic uses are (1) the representation to the integrating nervous system of bodily and environmental movement, each of both the active and the passive kinds. (2) Posture in a broad and general sense. (3) Stereognosis, the recognition of shape internal, within the hands or arms or legs, and external, in a room or along a devious pathway. (4) Appreciation of weight, weight of the external bodily parts as well as of external objects. (5) Maintenance of equilibrium, whatever the posture or the mode of personal locomotion. (6) Sensing of jolts, jars, and material vibrations coming from the environment. (7) Pressure and impacts other than jolts and vibrations. (8) Elaboration (and recording?) of the motor ideas through which the body is moved and controlled. (9) The spatial relationship of local sensations: local sign. (10) "Rhythmic control of the circulatory compression of the veins."

It never needs elaborate demonstration to convince students that the movement-sense so connoted involves continually practically every portion of the organism and with a variety of sensations or of afferent influences roughly indicated perhaps by the dozen or more histologic forms of receptors. Be the "variety" or the uniformity as it may be, here at all events is ample psychophysiologic country for survey, careful study, and cartography.

Cenesthesia proper, the sensation-fabric, ground at once of the subconscious and the fully conscious aspects of mind, is merely indicated in our class discussions and not at all described. It will bear, none the less, an indefinite but large amount of description. Some men are already beginning to practically realize this—clinical neurologists, psychopathologists, physiologists, and a few academic psychologists who have not remained under the cataleptic influence of the "five-senses" bugaboo. This is one of those traditional simple concepts that seize the common knowledge like an opiate and hold it shut for years or decades or centuries to the passing and evolution(!) of time. Vision and hearing and taste and smell are part, but only part.

Experience has shown that probably the easiest and certainly the surest way of reminding a student-class of their respective cenesthesias, is to suggest the somatic regions and systems where its mental aspect is most obvious. This category and *feeling-of-being-alive* comes in a flood from the body alive, as may be felt from the following suggestions. Until one has been dead, so that the contrast between living-cenesthesia and dead blankness and lack and mere materiality can be actually appreciated, personally and vividly, no one has sufficient sanction to categorically deny this flood of neurokinetic influences, dynamic index of the mind in its relationship to matter.

Richer than any other one region, probably, in cenesthesia is the *head*, partly because vision, hearing, taste, and smell are spatially placed here, but partly owing also to the surpassing complexity of structure and of function herein located: There is an elaborate vascular mechanism within and around

the brain, a hydraulic system with plenty of sensation related to it. There are muscular and articular strains of unequalled complexity, since the face is the great expression-organ of the personality; one thinks of the scalp, the eyes, the mouth particularly. The interior of the head below is rich with sensations other than those of taste and smell and movement proper; think of the extensive and complicated nasal fossæ, the eustachian tubes, the middle ear; the dental alevoli and the teeth themselves. Brain-action gives undoubted sensation whether its disputed nature be one or another; and "nerve-tire," cerebral fatigue, has a complex and strong sensation like no other. Atmospheric electricity occasions head-experience which needs elucidation further. These are some of the "sensations" coming from the head and neck.

As for abnormal cenesthesia (pain) in the head, A. Neuman devotes more than thirty pages to it in Behan's momentous treatise, "Pain," 1914, and many varieties and causes are described.

To the present writer it seems absurd and nothing less to ignore in one's philosophy a flood of afferent influences, sensory and subsensory, coming from regions as extremely complex as these. A better knowledge of anatomy and of physiology would make some college psychologists admit it, some for whom still cenesthesia and subconsciousness are largely sources of innocent merriment, so strong are the traditional chains.

The mechanical *respiratory apparatus* directly and indirectly involves the entire body, and the chemic respiratory interchange in another sense may possibly make dim but extensive contributions to the "common sensation." The properly kinesthetic factor of this is obvious, for it concerns the entire thorax and abdomen inside and out, and muscularly the legs and even the arms and neck. As is sometimes said, it probably makes larger donation to the pleasantness of being alive than does any other process whatever. In the nostrils, nasal fossæ, larynx, trachea, bronchi, possibly in the alveoli (over the afferent vagus) arise a host of influences for the nerve-centers, the complexity of which would be appre-

ciated only on detailed consideration of the very numerous different tissues and structures and processes involved. The abnormal cenesthesia of the respiratory relations is correspondingly various.

The *hemolymph circulatory mechanism*, "the circulation," has a larger confluence of afferent messages concerned with it than most psychologists realize. The normal conditions are chiefly related to emotion, of course, and in particular is the heart involved, especially in fear and in joy. The writer has been interested in the vaso-motor aspects of this particular phase of cenesthesia and is convinced that these material changes in the diameter and in the compressibility (due to blood-pressure changes) of the arteries (and perhaps of the veins?) play no small part in the common sensation, especially that which is on the frontier between a sense of fullness and one of unpleasantness (or pleasantness) from congestion verging on inflammation. Muscular and general atony, too, may have part of its felt languor, etc., from local or universal pressure-lacks within the tissues. In certain parts of the body, especially in the reproductive mechanism, these vascular pressure-differences are mentally conspicuous, so that their cenesthetic capability is certain. In the "rubor, tumor, 'thermor,' and dolor" (an ancient lesson in inflammation), the importance of the abnormal degrees of these reactions is obvious.

The contributions to cenesthesia arising in the heart itself seem to be chiefly of two sorts, those due to unusual heart-rate and to over-action, and those of an abnormal nature. Much of the sensation related to the cardium and its tensions is pain, endocarditis, acute dilatation, mitral incompetency, ganglionic degeneration, angina pectoris being among its commoner occasions. It is important to note however that Behan states explicitly that the "raising of the intraventricular tension often causes cardiac pains"; if pains "after pronounced exertion," why not pressure sensations from lesser degrees of tension, and (in theory) always?

The vagal 'depressor' influence, representing general vasomotor adaptation, seems to have no little weight in the

emotions, and to be a rather important factor in the mental aspect of moods and perhaps of temperaments. All such matters, at present dumb, need to be given scientific voice. For example, the surprisingly general "depression" of both mind and body late in the afternoon, euphoria depressed into dysphoria, by simply the absence of an accustomed cup of coffee at lunch. Much of such an effect has its affective basis apparently in some thing like a contrast-effect related to heart-action.

The *nutrition mechanism* is the seat of more cenesthetic consciousness and subconsciousness probably than any other system, partly because of its great size and considerable complexity and partly because morphologically it is largely a body surface, exposed therefore to part of the environment directly. This integrated set of vital implements has been studied, too, by people with psychologic interests more than has any other viscus, by the physiologists Beaumont, Cannon, Carlson, Crile, and observers abroad, and by a few psychologists, of whom the latest to report is E. G. Boring in the Psychological Review, July, 1915.

As has occurred occasionally in other respects, Boring's interpretation seems a bit less scientific than his research-results proper, as if possibly somewhat restrained by tradition and too little oriented in the light of certain pathological data. It is ungracious surely to suggest this, especially when just this kind of study of every portion of the organism is precisely what is most needed to revivify and stimulate our science to accurate description of things as they are and then to their satisfactory explanation.

The digestive and absorptive apparatus has a great richness of structure, of process, of nerve-supply of every kind, and of indirect mechanical relationship to the remainder of the torso at least.

This last can not be ignored in a discussion of this kind, since the psychologist is concerned with mental processes related to living practical processes as they actually are and not often with mere anatomic units which exist as units only to be inseparable parts of a living whole. Many a student

of physiology, of physical education, and of psychology, and many an instructor of anatomy, forgets this or never learns it, and is thereby misled into a wrong because partial view. 'Referred pain' is a case in point: the psychologist, like the clinician, is concerned chiefly with the practical place of origin, in a functional sense, of the pain, and not much with the spatial location of the local experience based on neural distribution.

The mouth is rich in sensory and 'emotional' data from the lips to the pharynx—months of questionary work would not exhaust this one little end of the alimentary canal termed by some unpoetic German the funnel of the stomach. The pharynx is relatively passive, yet with both vegetal and voluntary muscle in its walls. The esophagus, especially its lower, cardial end, has possibilities of rich sensation, chiefly painful but by no means wholly so. Consider then the twofold stomach, the pyloric valve, the small intestine, the pancreas, liver, gall-bladder, bile-ducts, the ileocecal valve, the appendix, the colon intimately connected with the stomach and with the supporting diaphragm and pericardium upward even to the shoulder-girdle. The sigmoid flexure and the rectum with its important sets of sphincters, reflex and voluntary, certainly have an important place in the consciousness as in the subconsciousness of the individual. The complicated movements of the intestine and the borborygmic events provide a considerable tributary to the stream of consciousness even under normal circumstances of mind and of bowel. When the mind is hypochondriacal, hysterical, or paranoic, and when the bowel is inflamed so that the movements and the secretions are vastly increased, or atonic so that there is constipation, there is positively no assignable end to the sensations to which the nutrition-mechanism proper may give rise in consciousness. Motor, secretory, neuritic, neuralgic, emotional processes are all normally concerned in this still unravelled cenesthetic complex, sensory and subsensory. The mere mention of nervous dyspepsia and hysterical anorexia suggest research work for several cand. doct. phil. for years to come. Why not appreciate that such work

would base the human mind on a firmer base than ever it stood on yet?

The *urinary apparatus* constitutes in a sense part of the nutrition mechanism, but its machinery is distinct. Kidney, ureter, valve, bladder, sphincters, urethra—each has its contribution of clear or of vague sensation in the fabric underlying our empirical minds, 'conscious' or ready-to-be-conscious, that is, subconscious. The urinary bladder especially and its outlet have a rich complex of sensory neurones and its movements, active, tonic, and really passive, have no small part in the emotive life at every age. Introspective analysis here would certainly reveal data worth the having, especially if pathologic conditions, hysterical and neurotic, were included in the research to furnish exaggerations of the usual and normal contributions, and if here, as elsewhere, anesthesias, by way of negative variation, could be observed. The tonus of the urethral sphincters respectively in the two sexes plays a part in adolescent, as in infantile, consciousness and merges oftentimes into the still richer field for exploration by psychology, the reproductive mechanism.

The *genital organs*, external and internal and mammary, have been elaborately studied of late in certain of their relations to society and to criminology, but even the start in the purely cenesthetic analysis as a basis for the rich love-aspect of humanity has scarcely as yet been made. One thinks first of course of Ploss and Bartels's 'epochal' work ('Das Weib'), of Havelock Ellis's 'Psychology of Sex,' and of Moll's 'Sexual Life of the Child.' The present writer has started the cenesthetic analysis for his annual lectures in the School of Eugenics on the technical psychology of the sexual impulse. Here certainly is a rich field, hard and often disagreeable to till (the motives of the tiller often misjudged), but with material that involves more than any possible other the evolution of the mind as a social reagent. In general only the keen family physician, the really learned gynecologist, the scientific social worker, and the psychopathologist above all realize how rich this relationship is here between physiologic process and womanhood and manhood in the realest sense of these words.

The *spleen* has abundant trabeculæ of smooth muscle with known active movements of contraction, and it is proper therefore to suppose sensory or at least subconscious concomitance. It has nerve-connections with the vagus and with the splanchnics, both having afferent pathways. Distension of the capsule increases the sensorial tonus and strain until actually painful.

The *skin and mucosæ*, more than two square meters in extent, are cenesthetically as well as kinesthetically productive. These I have recently discussed somewhat in this journal.[1] The skin has the epicritic sensitivity as well as the protopathic beneath it, and contributes always a large but variable amount to the stream of consciousness, especially of course in its (intrinsic and extrinsic) environmental phases. Heat and cold, the tickle of excessive sweat and of other agents, the movement or stagnation of the air, massive friction and pressure sensations, hair-erection (reflex or voluntary), hyperemia from vasodilation,—all these and others constitute a rich complex of actual or potential consciousness.

The mucous membranes, inasmuch as simpler in structure and function than the skin, have simpler cenesthesia, but their modicum is none the less important for descriptive psychology. Let the reader for example attend a moment to the "feel" of his conjunctiva or of the back of his tongue and thus be reminded of the things familiar *always* to the subconscious bulk of his mental process.

The *epithelia*, gland-tissues, have in some cases, at least, essential part in the cenesthesia, especially in connection with their respective mucosæ, but also by themselves. For the former one cannot avoid thinking of the salivary secretion and of the genital. As for the glands themselves, the thyroid and thymus and mesenterics (Behan) have distension-sensations and pain well defined, while the adrenals furnish warning of hemorrhage by severe pain. The breasts are rich in sensations both pleasurable and painful, and are provided with many other sensations at different stages of life. Action of many of the other glands is distinctly pleasant.

[1] "Certain Further Factors in the Physiology of Euphoria," XXI., 3, May, 1914.

Pathologically, the sensory complex of the secretory system is a rich one, with always a "limit" (on one side of the affective balance) of pain.

The *osseous and connective tissues* give rise, especially the former, to numerous grades of sensation, but these, like the sensory quota of the far more important synovial membranes, are properly classed as kinesthetic, because directly motor.

In an organic afferent world thus sketchily suggested, composed of all our receptive fields, certainly there is unlimited room for raising a good quality of mind!—and the cultivators thereof will prosper.

This sensation-fabric, especially its kinesthesia, personified, made useful by relation to the environment within and without, mediately gives us the *meaning* of bodily life because it is the dynamic index in our personalities of "the world we live in." At least thirteen hundred thousand pathways, according to one count and estimate of a reliable grade, connect all these regions (through the autonomic, partly) with the spinal white or grey and there through with the great cortex; and no man knows yet and no woman can guess how many neuronal paths that more or less represent mind are part of the autonomic itself. Perhaps[1] our search for psychophysical couplers, such as neurones, is entirely superfluous anyway; personally at present I do not go so far. Besides the neurones, then, which bear influences (impulses or perhaps strains) into the cerebrospinal axis, at least seven hundred thousand (by the same count) carry messages outward from it, and it would be both pedantic and wholly unwarranted to assert that these motor influences make up no part of the process of normal mentation.

It is theoretically important to observe in passing, as further 'elaboration' of what should long since have been 'the obvious,' that this double flood of influences afferent and efferent in one way or another *involves the action of every part of the brain.* We came to the same conclusion in a discussion of voluntary movement about four years ago, and unpub-

[1] See "Consciousness in the Brutes," *J. of Nerv. and Mental Dis.*, XXXIV., 1 and 2.

lished work on the physiology of emotion and on the neurology of thought lead to the same necessity of belief.

A deep knowledge of cenesthesia would have value not only in the science of psychology, but properly developed and then properly sifted and subjugated to the hygienic requirements, it would have practical educative and therapeutic values of account to every individual, perhaps especially in its kinesthetic phases, but also in the other phases as well. Most of us have never explored even the living executive house in which we live, although to know it thoroughly (but neither too thoroughly nor too habitually) is the indispensable basis of that self-control that means personally much. Such knowledge would also be the basis of that generalized skill ever richer that means to most of us 'socially' nearly everything, for on it almost always depends a mortal livelihood. To develop, in short, a real scientific knowledge of what I have tried to make explicit as cenesthesia would be to make a man, in one sense for the first time, master of himself (if not indeed 'captain of his soul'). Then at length will motives be less blind—and perhaps virtue, as well.

But cenesthesia certainly is not mind, for an unexplained remainder intervenes between this feeling-of-being-alive and the subconsciousness. This unexplained remainder has been variously explained, but never better, because never more vaguely, than by the Sage of Königsberg as "the transcendental unity of the understanding"; perhaps were I an older convert to Animism than I am, we should read instead of this sonorous phrase simply the mind in process, or perhaps the Soul.

At any rate we have arrived at the subconsciousness, and here description becomes needless, for you all are familiar with 'The Philosophy of the Unconscious' by Eduard von Hartmann, with Morton Prince's 'The Unconscious,' and with many descriptive works between these and before. You all certainly are familiar in some way with the massive part of the stream of mind and with this part which especially gives it momentum, making it go and making it *do*. Knowledge

of all this, certified and vivified of late by the new Freudian books and the work of physiologists typified by Cannon, has been in the philosophic atmosphere for long.

One of its names in the history of philosophy is Transcendentalism, in some respects the culmination of practical philosophy up to our time. One thinks of Carlyle, of Browning, and, here in the New World, first especially of the great Emerson. Self-reliance, compensation, 'circles,' even the 'Over-soul' itself finds its expression at least in the new philosophy of the subconscious. "If we ask," says Emerson in 'Self Reliance,' "whence this ['immense intelligence'] comes, if we seek to pry into the soul that causes, all philosophy is at fault. Its presence or its absence is all we can affirm. Every man discriminates between the voluntary acts of his mind, and *his involuntary perceptions, and knows that to his involuntary perceptions a perfect faith is due. He may err in the expression of them, but he knows that these things are so, like the day and night, not to be disputed.*"

This certainly expressed the subconscious phases of the human mind and even makes explicit (in the 'involuntary perceptions') the sensational flood, the cenesthesia as index of the reactions of the organism. When one considers the height from which Emerson viewed life, as far as possible 'above' neurology and technical psychology, the evidence is still more striking.

And yet, psychologists in general are certainly not yet awake to the deep and wide implications of the subconscious, although many are dreaming of them (in their subconscious). How can one awaken them? for surely they belie not only their own breadth of mind but their science in keeping their brains, like their eyes, shut tight.

What possible sanction have they to drowsily suppose they may dream on, a dream within a dream? To do so would be comparable to the behavior of a self-taught physician if he refused to accept the infectious origin of many diseases, or to use an anesthetic in his amputations, or to recognize the therapeutic uselessness and menace of alcohol. Such a one would be himself a menace to the common welfare. But surely

the teacher of psychology who refuses to include the subconscious aspects of mind concomitant to reaction and cenesthesia is a like offender, although one far less baneful. But he is misleading his students,—a modern Sophist 'making the worse appear the better reason' for many kinds of human behavior.

Psychology is as perfectly fitted to be one of the most essential, most interesting, and most practical subjects of every grammar-school curriculum, as to be the field of research in work for the philosophic doctorate. But without the subconsciousness as a basis it cannot easily be made generally explicable.

Some of you certainly are saying, Surely each philosopher has his right to his own opinions!? I protest he has not when those opinions unduly narrow a science and deprive it of half its interest and more than half its force. None are so deaf, traditionally, as those who will not hear! Think you that some of the neurologists and the psychopathologists and the alienists are not competent psychologists because they deal partly with mind that is out of order? It is time that such mediæval crudities of distrust were outgrown.

It certainly is only by such continual and immediate reference to the body, the mind's own sister organism, that the great goal of the philosophy of mind, real explanation of human motivity, can ever be attained. Perhaps then at length our science, determinant in various degrees of all the rest, will take her rightful place as the Queen of the Sciences in the unanimous estimation of the world of men and women whom, in part, it will then really explain.

Summary

Laboratory research in psychology, much as it has done, has failed to *explain* the mental process. Far too little time has been devoted, perhaps, to understanding the immediate relations between the body as an organism, at once developer and adapter of the soul, and the mental series proper. We need much more casuistric introspection based on anatomy

and physiology; recent advances demonstrate anew the reality and urgency of this lack.

The organism by its inherent nature is ever in universal movement, movement both active and passive, and continuous both in time and in space. The (1) universal muscular tonus; (2) equilibrium; (3) reaction-basis; (4) the reciprocal innervation of functional antagonists; (5) irradiation; (6) volition; (7) reflexion; (8) mechanical respiration; (9) circulation; (10) digestion; (11) excretion, are some conspicuous topics under which this universal organic motion, always unique, might be examined into. To deny as 'unproven,' 'illogical,' etc., the immediate relation of this universal movement with mind, is little short, these days, of jugglery with the truth, denying both at once the very ground of the whole category Life and its final purpose.

Cenesthesia is the sensory and subsensory aspect of this universal bodily movement, director of the soul's important business. Its details in every portion of the organism should be known to every psychologist as the very elements of his subject. This sensation-fabric lends the meaning to life because it is in an important sense the dynamic index of our personal evolution and of our adaptation to our kinetic surroundings, both material and spiritual. Its reality is not lessened by its necessary partial *action in the dark.*

The practical educative prophylactic and therapeutic values of cenesthesia are great, but as yet are relatively undeveloped save in just the wrong direction (hypochrondria).

The new work of the psychopathologists and of the physiologists lends new dynamism to the subconsciousness, to which by integration of the cenesthesia we arrive. However much *more* it may mean, the transcendental 'over-soul' expresses this phase of the mental process.

To ignore the subconsciousness as an element of mind in one's thought, and especially in teaching, is as unsanctioned as for a physician to refuse the fact of the infectious origin of some diseases or of the menace of 'therapeutic' alcohol; it is to try, once more, to 'make the worse appear the better reason.'

Psychology is perfectly well fitted to be a popular and essential grammar-school subject.

These propositions are not yet 'massive' ideas to most psychologists, but there is great need that they become so.

It may be that only by elaborating this moto-cenesthetic relationship can psychology become really explanatory and so take her rightful place as the queen of the sciences.

MENTAL ASSOCIATION FROM PLATO TO HUME[1]

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I

The notion that one idea or memory image follows another according to certain definite principles was the first step toward a general theory of association among mental states. This fundamental notion is found first, and in quite definite form, in the writings of Aristotle (B.C. 384–322). The earlier Greek philosophers were too engrossed in the problems of objective reality to examine the processes of experience.

Plato suggests almost casually, in the *Phædo*, the function of *contiguity* and *similarity* in the act of recollection. "What is the feeling of lovers when they recognize a lyre or a garment or anything else which the beloved has been in the habit of using? Do not they from knowing the lyre form in the mind's eye an image of the youth to whom the lyre belongs? And this is recollection: and in the same way anyone who sees Simmias may remember Cebes. . . . And from the picture of Simmias you may be led to remember Cebes. . . . Or you may be led to the recollection of Simmias himself. . . . And in all these cases the recollection may be derived from things either like or unlike. . . . When we perceived something either by the help of sight or hearing or some other sense, there was no difficulty in receiving from this a conception of some other thing, like or unlike, which had been forgotten and which was associated with this."[2]

Neither here nor in the 'Timæus,' which contains a fairly broad psychology, does he develop the notion further. According to Plato, knowledge is innate; learning consists merely in drawing out such knowledge as the soul had in

[1] Part of a forthcoming volume on the history of Associationism.

[2] 'Phædo,' 73–6, Jowett trans.

previous existences. We draw out such knowledge, just as we recollect events of the present life, by means of similar experiences, or by unlike but 'contiguous' ones.

Aristotle's systematic investigation of the whole realm of things knowable led him to examine mental facts quite as thoroughly as physical phenomena, and to employ for this purpose considerable self-observation. In the course of his study he noticed that the links in chains of thought which end in the recollection of particular facts succeed one another more or less systematically, and he believed that he had discovered the principles of their connection. Only three sorts of relationship, he affirms, are involved in the succession of thoughts: similarity, contrast, and contiguity. This and his doctrine of the fusion of experiences mark the historical starting-point of Associationism.

Aristotle's analysis of the sequence of mental processes is somewhat obscure, owing partly to the physical import of many of his terms, partly to the difficulty of getting at his real standpoint and interpreting his statements into modern psychological language. There are uncertainties too in the text at critical points, and one is always in danger of reading into his elliptical phraseology a meaning which he may never have intended. Sir William Hamilton's brilliant and exhaustive discussion of Aristotle's contributions to our theme is certainly open to this last objection.

It is in treating of memory and recollection that Aristotle brings out his notion of association. "A sense is capable of receiving into itself sensible forms without their matter, just as the wax receives into itself the mark of a ring without the iron or gold."[1] The persistence of such an impression constitutes memory,[2] which is 'neither sensation nor conceptual thought but the condition or modified form of one of these after the lapse of time.'[3] "Memory is the possession of an experience potentially revivable."[4]

The problem then arises, How is such a revival, which we

[1] 'De An.,' II., Ch. XII., 1; *cf.* 'De Mem.,' I., 10.

[2] 'De Mem.,' I., 10.

[3] 'De Mem.,' I., 4.

[4] 'De Mem.,' II., 12.

call recollection, brought about? "Recollection occurs," says Aristotle, "inasmuch as one experience naturally succeeds another. If [the succession be] necessary, it is plain that when the one is stimulated it will stimulate the other; if not necessary but habitual, then it will stimulate it only in most instances. It is a fact that some persons are more habituated after being stimulated once than others after many times; just so we remember some things better after seeing them once than other things after many times. Hence, when we are recollecting we keep stimulating certain earlier experiences until we have stimulated one which the one in question is wont to succeed. And just so we hunt through the sequence, thinking along from the present or some other [thought], and from similar or contrasted or contiguous. By this means the recollection comes; for the experiences are in some cases identical with [the one in question], in others simultaneous with it, in others they involve a portion, so that the remainder is small and is thereupon stimulated. In this way, then, persons make effort [to recollect], and in this way also without effort they do recollect, when the [experience] in question succeeds some other experience; though it is generally after a succession of other experiences such as we have mentioned that the one in question comes. It is not necessary to inquire how we remember the remote, but only the contiguous; for it is plain that the procedure is the same—I mean, that of sequence—when we [recollect] without preliminary effort and even when we fail to recollect. For experiences habitually follow one another, this succeeding that. And so, when a person wishes to recollect, he will do this: he will endeavor to get some initial experience, which the one in question succeeded. Just so recollection comes most quickly and best [if it proceeds] from an initial point; for just as things are related to one another by sequence, so also are experiences; and such matters as have a regular order are easily remembered—for instance, mathematical science; others poorly and with difficulty."[1]

[1] 'De Mem.,' II., 6–11 (451*b* seq.). I am indebted to my colleague, Professor E. Y. Robbins, for assistance in this translation, which aims to go no further than

The conception of association developed in this remarkable passage covers a broader field than the mere act of recollection which it seeks to explain. Aristotle's main contentions may be restated in more modern form as follows: In accounting for recollection we should observe that it depends on the *sequence* of mental processes. Mental processes always take the form of a series, or train. Their serial association is due in some cases to a necessary connection, in others merely to their occurring together habitually. Habitual connection brings about actual association in most cases, but not always. Its power to do so varies with the individual and the sort of experience. The series may start with present experience (? sensation) or with something besides present experience. Its members follow one another according to similarity, contrast, or contiguity. The process is the same in 'efforts to recollect,' whether successful or not, as in spontaneous recollection. Usually the series or train consists of several members. After some elaboration of the last two points the author makes this practical application: Since *serial order* is the natural characteristic of mental states, we shall recollect most readily what we wish if we begin by calling up something related to it; and similarly, subjects which have a natural order of sequence, such as mathematical truths, are those most easily fixed in memory.

Aristotle's contribution to the theory of successive association, if this interpretation be correct, is fourfold. (1) He was the first to point out clearly that the sequence of cognitive experiences is not mere chance, but occurs through a definite process of natural association. (2) He recognizes habit as an important factor in determining association—subject, however, to individual variations. (3) Most important of

the text justifies. The words in square brackets are our own insertions. Aristotle's use of physical terms in discussing mental facts will be understood if we render κίνησις *stimulation*, instead of *experience;* the verb κινέω has been translated *stimulate*. Aristotle uses the noun, like the verb, in the sense of movement or energy, but psychologists will find 'experience' or 'mental process' quite near his meaning—and more intelligible. The text used is Bekker's edition. Compare Sir Wm. Hamilton's rendering (Reid's Works, Note D**), and the excellent translation by W. A. Hammond ('Aristotle's Psychology,' p. 204). I have followed Hammond's version in the other quotations.

all, he specifies the three principles of similarity, contrast, and contiguity as the sole bases of 'habitual' associative connection. (4) He declares that the same laws hold in purposive thinking as in the spontaneous flow of thought.

Fusion, or simultaneous association, was not altogether overlooked by Aristotle, though his discussion of this point contains more apriori reasoning than introspection. Two separate simultaneous sensations, he says, are impossible. Even with a single sense, such as vision, the stronger of two sensations will displace the weaker; or if they be of equal strength they will counteract each other, leaving no sensation at all. But if they are not separate, they may combine and a new sensation arise, fused out of both elements.[1] Much less can there be two separate simultaneous sensations from different senses, such as vision and taste.[2] But in this case also fusion may occur, as for instance between white and sweet. This variety of fusion is attributed by him to 'some unitary principle of the soul,' which senses the white and the sweet as a single unit.[3] It is interesting to note that very similar conclusions regarding fusion are reached by a set of modern writers—based, however, on quite different premises.

II

I can find no evidence that Aristotle's views on successive association were adopted or even understood by the thinkers of the centuries immediately following. In a few instances only was the problem even approached.

The psychology of the Stoics we know only at second hand. According to the summary of their doctrines given by Diogenes Laertes (ca. 200 A.D.), which is the nearest we can come to the original, the Stoics held that thoughts are formed by "similarity, or analogy, or transposition, or combination, or opposition. By a direct perception we perceive those things which are the objects of sense; by similarity, those which start from some point present to our senses; as, for instance, we form an idea of Socrates from his like-

[1] 'De Sensu,' VII., 3–4; *cf.* 7.

[2] *Ibid.*, 9, *cf.* 14.

[3] *Ibid.*, 24, *cf.* 26.

ness."[1] It is not clear whether this view was held by Zeno of Cittium, the founder of the school (B.C. ca. 342–264), or was developed by his disciple Chrysippus (B.C. ca. 281–208). The examples which are given of the other processes indicate a very superficial analysis, which does not touch the problem of the modes of association.

Epicurus (B.C. 342–270), according to the same authority, held that 'every notion proceeds from the senses, either directly, or in consequence of some analogy, or proportion, or combination.' 'The recollection of an external object often perceived anteriorly' occurs as follows: "At the same moment that we utter the word *man*, we conceive the figure of a man, in virtue of a preconception which we owe to the preceding operations of the senses."[2] The notion of successive association is certainly implied in this, but it is not clearly brought out, and no analysis of the ways and means is given in the text. Nor do the disciples of Epicurus appear to have worked out the problem further.

Among the Sceptics, Carneades (B.C. 213 ca.–129) definitely alludes to successive association. According to Sextus Empiricus, this writer compares the succession of thoughts to a chain, in which one link is dependent on another.[3]

St. Augustine (A.D. 354–430), though indebted to Aristotle, takes a different view of memory and recollection, in which he lays most stress on association by *contiguity*. Memory, he says, receives within its gates all sorts of sense impressions. "Nor yet do the things themselves enter in, only the images of the things perceived."[4] Out of this store we are continually weaving together new images of things which we have experienced (§ 14). In darkness and silence we can bring up colors and sounds (§ 13). "But what when the memory itself loses anything? . . . Where do we search but in the memory itself? And there, if one thing be offered instead of another, we reject it, until what we seek meets us;

[1] Diog. L., VII., Zeno, § 36; Bohn's trans., p. 278.
[2] Diog. L., Bk. X., § 21; Bohn, p. 435–6.
[3] Sext. Emp., 'Adv. Math.,' Bk. VII., § 176; see Hamilton's 'Reid,' p. 894.
[4] 'Confessions,' Bk. X., § 13; Parker's trans., 1885.

and when it doth, we say 'This is it.' . . . By the part whereof we had hold was the lost part sought for."[1] That the "lost part," when it is found, comes from memory, is shown by the fact that it is recognized. If we are trying to recollect a name, and some one mentions it, it comes to us not as something new, but as familiar. "Were it utterly blotted out of the mind, we should not remember it, even when reminded."[2]

Augustine limits the associations involved in recollection to relations of contiguity; the experiences reinstated are 'parts' of the experience which calls them up. But as his illustration shows, the relation of parts is not limited to mere spacial contiguity; it embraces the totality of elements in any experience. He applies the principle of contiguity in its broadest sense.

The many commentators on Aristotle during the middle ages took up the passage on recollection which I have quoted. They discussed and amplified it, as they did every saying of the master, but without throwing any new light on association. Sir William Hamilton made an exhaustive search of this literature, which he presents in the voluminous note (D**) at the end of his edition of Reid's works. In none of the authors there cited, except those already mentioned, can I find any real contribution to the doctrine up to the time of Descartes and Hobbes. It must be borne in mind that during the latter part of the middle ages to depart from the dicta of Aristotle was regarded as akin to heresy. Any freshness or originality was frowned upon; the only advances came from new interpretations—and these too often were misinterpretations. The verbal contributions to the doctrine of association during this period are minutely treated in Hamilton's survey, to which I refer the reader who delights in textual exposition.[3] A single instance will serve to illus-

[1] § 28.

[2] *Ibid.*

[3] Hamilton's citations are reliable, but not his interpretations of other writers. His criticisms are biased by his own standpoint and often reflect his prejudices. As I have already said, he reads too much into Aristotle's elliptical phraseology. I am also of the opinion that he gives altogether too little credit to Hobbes and other writers

trate the vain groping after new light at the dawn of modern times.

Luis Vives (1492–1540), a Spanish commentator on Aristotle, was at the same time a writer of some originality. Commenting on the passage which we have examined, he ventures the view that recollection occurs 'by steps,' 'from cause to effect; from the latter to instrument; through the part to the whole; from this to situation; from situation to person; from person to earlier and later things; to contrary; to similar.'[1] "On looking at a place, from this there comes into the mind what we know to have happened in the place or to be situate there."[2] He thus recognizes the broad fact that trains of thought proceed in the direction of many sorts of logical relationship; but he does not attempt to analyze these laws of association into their first psychological terms as did Aristotle.

René Descartes (1596–1650), the herald of the new era of thought, in spite of the great variety of topics which he treated, has little to say on the subject of memory and recollection. We may attribute this neglect to his doctrine of innate ideas, which made it seem to him of slight importance to explain how former experiences are 'recalled' in memory.

In his posthumous work, the 'Traité de l'homme,' speaking of the action of objects on the nerves and brain, he says: "They have the strength to form for themselves there certain passages which still remain open after the action of the object . . . has ceased, or which, if they close up, at least leave a certain disposition in the little fibers of which the brain . . . is composed, by means of which they can much more easily be opened again than if they had never been so before. . . . And also it must be noticed that if we open only certain ones . . . this of itself may be the cause of others . . . reopening

of later date. Originality consists not always, nor chiefly, in collecting the raw material; more important is the shaping of the material—the framing of a consistent viewpoint. A doctrine is not old because its elements have been used before, any more than a poem is old because previous writers have employed the same words or the same alphabet.

[1] 'De Anima,' Bk. II., ch. De Mem.

[2] *Ibid.*, Bk. I.

also at the same time, especially if they had all been opened together several times and had not been used to being so one without the others. This shows how the recollection of one thing may be excited by that of another which was formerly impressed on the memory at the same time as it."[1] "The vestiges in the brain," he says elsewhere, "render it fit to move the soul in the same fashion as it was moved before, and thus to make it remember some thing, even as the folds which are in a piece of paper or a cloth make it more fit to be folded as it was before, than if it had never been so folded."[2]

Descartes seems to hold that things which have been experienced together tend to be recalled together (contiguity), and that an experience will recall an earlier experience which partially—even though not wholly—resembles it (similarity). The reinstatement is made easier through habit, and is accomplished through the 'vestiges' of the former experience in the brain.

III

With Thomas Hobbes, of Malmesbury (1588–1679), began a new epoch in the development of the doctrine. Aristotle's work was mainly by way of enumeration and classification. He applies the laws of associative connection which he discovered to but one class of experiences—memory. Only by implication are they extended to the relations between memory, sensation, and the other mental processes. And neither the later Greeks nor the Schoolmen, neither Augustine nor Descartes, despite their independent analyses, advanced perceptibly beyond his position. Augustine merely emphasizes the law of contiguity at the expense of similarity and opposition. For Descartes the associative process signifies a connection between brain states rather than experiences.

Hobbes's psychology is noteworthy as an initial attempt (1) to establish the relation between different sorts of mental

[1] Cousin's French ed., Vol. IV., pp. 400–1. The omissions, with one exception, are letters referring to an accompanying diagram. The translations from Descartes are mine.

[2] 'Lettres,' I., 36; French ed. of 1667.

states on what we would today call a psychophysical basis; and (2) to trace all mental content ultimately to sense experience, doing away with the old notion of innate ideas. The real importance of Hobbes must be measured in the light of his influence on subsequent thought. The British thinkers who followed him developed their systems of psychology along the lines that he marked out; the notion of association, which he did little more than outline, became more and more prominent as the analysis was perfected. His historical value to associationism, therefore, lies not so much in his own contributions to the doctrine as in the fact that he established the type of psychology out of which it naturally and logically developed.

Hobbes's own standpoint is sensationalistic, rather than associationistic. He is chiefly concerned in showing that the 'cognitive powers' of the mind deal only with material given by the senses. The effect which an object produces on the brain, he says, does not cease when the object ceases to work upon us; 'though the sense [sensation] be past, the image or conception remaineth.'[1] Imagination is any 'conception remaining and by little and little decaying from the act of sense.'[2] The strongest and clearest of these images are found in dreams and in visual after-images.[3] A weaker sort is fancy, which he calls *fiction:* "The brain, or spirit therein, having been stirred by divers objects, composeth an imagination of divers conceptions that appeared single to the senses. As for example, the sense showeth at one time the figure of a mountain, and at another time the color of gold; but the imagination afterwards hath them both at once in a golden mountain."[4] Imagination in all its forms is 'nothing but decaying sense.'[5] "Imagination and memory are but one thing."[6] Expectation is of the same nature; for, 'of our conceptions of the past we make a future.'[7] Even science,

[1] 'Human Nature,' 1650, Ch. 3.
[2] *Ibid.*
[3] *Ibid.*
[4] *Ibid.*
[5] 'Leviathan,' 1651, Ch. 2.
[6] *Ibid.*
[7] 'Hum. Nat.,' Ch. 4.

the 'knowledge of the truth of propositions, and how things are called, . . . is remembrance.'[1] All the 'cognitive powers' are thus brought into relation, directly or indirectly, with sensation. "There is no conception in a man's mind which hath not at first, totally or by parts, been begotten upon the organs of sense."[2]

The succession of experiences remains to be accounted for. Sensations arise from the action of external things upon our organs,[3] and their succession requires no special psychological explanation. The sequence of imaginations is governed by sensation: "We have no transition from one imagination to another, whereof we never had the like before in our senses. . . . Those motions that immediately succeeded one another in the sense continue also together after the sense; insomuch as the former coming again to take place, and be predominant, the latter followeth by coherence of the matter moved. . . . But because in sense, to one and the same thing perceived sometimes one thing sometimes another succeedeth, it comes to pass in time that in the imagining of any thing there is no certainty what we shall imagine next; only this is certain: it shall be something that succeeded the same before at one time or another."[4]

Trains of thought are of two different sorts: (1) Such as are 'unguided, without design, and inconstant';—yet even in these, he remarks, we 'may ofttimes perceive the way of it and the dependence of one thought upon another';—and (2) those 'regulated by some desire and design.'[5] Regulated trains of thought occur, either 'when of an effect imagined we seek the causes or means that produce it,' or 'when, imagining anything whatsoever, we seek all the possible effects that can by it be produced.'[6] The sequence from effect to cause is

[1] *Ibid.*, Ch. 6.

[2] 'Lev.,' Ch. 1.

[3] 'Lev.,' Ch. 1.

[4] Ch. 3.

[5] *Ibid.* In the 'Human Nature' the trains are called 'casual and incoherent' and 'orderly' respectively. In this work also Hobbes attributes the succession of conceptions to their previous 'coherence or consequence' as sensations (Ch. 4).

[6] *Ibid.*

common to man and beast, while that from cause to effect belongs to man alone.

Hobbes thus concludes that *contiguity* in previous experience, which he terms 'coherence,' is the basis of all sorts of sequence of representations, but he finds it impossible in many cases to explain the preference of one association over another. Desire is a powerful agent in effecting the selection,[1] and so is habit.[2] Of the influence of habit on the succession of imaginations Hobbes says: "It is the nature of almost every corporal thing, being often moved in one and the same manner, to receive continually a greater and greater easiness and aptitude to the same motion, insomuch as in time the same becometh so habitual that, to beget it, there needs no more than to begin it."[3] Thus, man having invented language, "custom hath so great a power that the mind suggesteth only the first word, the rest follow habitually and are not followed by the mind."[4] It is interesting to notice that Hobbes emphasizes our lack of attention to the subsequent terms of an habitual association train, in the same passage in which he insists on the strength of the cohesion between the members.

There is only a brief allusion to fusion of images in Hobbes's writings. In the passage on *fancy* already quoted he recognizes the possibility of joining together in one imagination two images which the senses have given at different times. But he appears to regard the 'coherence' of sensations as a physical rather than a mental phenomenon.

IV

To John Locke (1632–1704) belongs the credit of originating the phrase *association of ideas*. Locke's 'Essay concerning Human Understanding' is avowedly an attempt to combat the Cartesian theory of innate ideas by showing that knowledge arises from experience alone. It was important for this purpose that he should examine thoroughly the relations between different sorts of experience and the processes by

[1] 'Lev.,' Ch. 3.

[2] 'Hum. Nat.', Ch. 5.

[3] *Ibid.*

[4] *Ibid.*

which one experience leads to another. It should be borne in mind that Locke uses the term *idea* to denote any sort of experience. He has no single term to express representative experience, to which the term idea was later restricted.

Locke distinguishes between two sources of ideas, sensation and reflection, and calls the two classes of experience that arise therefrom *ideas of sensation* and *ideas of reflection*, respectively.[1] Compared with Hobbes, he takes a more 'functional' attitude. Hobbes regards sensation as the source of all experience; the representative elements are merely corruptions of the original sense elements. According to Locke, the 'ideas of sensation' are transformed into other sorts of ideas by a separate function or faculty, which he calls reflection; and further, there are certain ideas derived from reflection alone; among the latter are the ideas of the processes of 'remembrance, discerning, reasoning.'[2]

Ideas, or experiences, are fixed in memory, he says, by *attention* and by *repetition*, but still more effectually by the *accompaniment of pleasure and pain*.[3] The reappearance of ideas as recollections is sometimes voluntary, sometimes passive. "The mind very often sets itself on work in search of some hidden idea, and turns as it were the eye of the soul upon it; though sometimes too they start up in our minds of their own accord and offer themselves to the understanding; and very often are roused and tumbled out of their dark cells into open daylight, by turbulent and tempestuous passions."[4]

Locke has little to say regarding their mode of sequence, whether by contiguity or similarity. In a passage often quoted he distinguishes between ideas that 'have a natural correspondence and connection with one another' and 'another connection of ideas wholly owing to chance or custom.'[5] It would not be difficult to resolve the former connection into similarity and the latter into contiguity. But Locke does not take the step. Elsewhere he says: "There comes

[1] 'Essay,' 4th ed., 1700, Bk. II., Ch. 1, § 24.

[2] *Ibid.*, Ch. 6.

[3] Ch. 10.

[4] Ch. 10, § 7.

[5] Bk. II., Ch. 33.

by constant use to be such a connection between certain sounds and the ideas they stand for, that the names heard almost as readily excite certain ideas as if the objects themselves which are apt to produce them did actually affect the senses."[1] This again implies association by contiguity, but Locke's purpose in the discussion is only to emphasize the strengthening effect of habit. Nowhere does he name or formulate the principles of the sequence of ideas, as Aristotle did.

On the other hand, Locke makes much of the phenomenon of simultaneous association, though his contributions to this phase seem to have been generally overlooked. The mind exerts its power over its simple ideas, he says, chiefly in three ways: (1) 'Combining several simple ideas into one compound one,' which gives us our *complex ideas*. (2) 'Bringing two ideas, whether simple or complex, together, and setting them by one another, so as to take a view of them at once, without uniting them into one,'—which gives us *ideas of relations*. (3) 'Separating them from all other ideas that accompany them in their real existence; this is called abstraction,' and is the means by which all our *general ideas* are formed.[2] These processes correspond closely to our modern conception of fusion, complication, and analysis.

The unity of the complex idea arises "from an act of the mind, combining those several simple ideas together, and considering them as one complex one consisting of those parts," even where they do not exist together in nature.[3] "All our complex ideas are ultimately resolvable into simple ideas, of which they are compounded and originally made up, though perhaps their immediate ingredients . . . are also complex ideas."[4]

In another passage Locke gives the name of *composition* to the operation whereby the mind 'puts together several of those simple ideas it has received from sensation and reflection, and combines them into complex ones.'[5] "As simple

[1] Bk. III., Ch. 2, § 6.
[2] Bk. II., Ch. 12.
[3] Ch. 22, § 4.
[4] *Ibid.*, § 9.
[5] Ch. 11, § 6.

ideas are observed [by the senses] to exist in several combinations united together, so the mind has a power to consider several of them united together as one idea; and that not only as they are united in external objects, but as itself has joined them together."[1] "The mind . . . arbitrarily unites into complex ideas such as it finds convenient; whilst others that have altogether as much union in nature are left loose, and never combined into one idea, because they have no need of one name. It is evident, then, that the mind, by its free choice, gives a connection to a certain number of ideas, which in nature have no more union with one another than others that it leaves out."[2]

In view of this rather extended discussion of simultaneous association, it appears certain that Locke had in mind the coëxistence as well as the sequence of ideas when he wrote the historic chapter on Association.[3]

According to Locke's view association, whether concerned with the succession of ideas or their composition into complex experiences, is of two sorts: it is based either on their 'natural correspondence,' or on 'chance or custom.' The latter sort may be voluntary or involuntary, and is that which leads us into all kinds of error in thinking. The fact of association, the union or nexus between different experiences, though an important factor in his psychological analysis, is not the chief factor in his psychology, since it shares the field with other fundamental principles. Yet it requires only a little deeper analysis of his material to bring out explicitly the laws of similarity and contiguity.

Locke seconds Hobbes's attempt to derive all complex experience from simple experience, but differs with him in deriving simple experience from reflection as well as from sensation. He thereby abandons the field of pure sensationalism; and in this respect he is followed by later English writers. His chief historic merit in psychology, however, is his contribution of the term *association of ideas*, which focused the attention of future thinkers on this factor as a means for

[1] Ch. 12, § 1.
[2] Bk. III., Ch. 5, § 6.
[3] Bk. II., Ch. 33.

the empirical derivation of knowledge. His emphasis of habit or custom as a factor in association also had considerable influence in determining subsequent analysis.

V

Bishop George Berkeley (1685–1753) lays considerable stress on the associative process, which he terms *suggestion*, and enumerates several of its modes. "Distance," he says, "is suggested to the mind by the mediation of some other idea, which is itself perceived in the act of seeing."[1] "That one idea may suggest another to the mind, it will suffice that they have been observed to go together, without any demonstration of the necessity of their coëxistence."[2]

Berkeley divides our mental content into ideas of sense and ideas of imagination. "The ideas of sense are more strong, lively, and distinct than those of the imagination; they have likewise a steadiness, order and coherence, and are not excited at random . . . but in a regular train or series."[3] The ideas of imagination "are more properly termed ideas or images of things which they copy or represent"[4]—a hint toward our modern use of the term *idea.* "I find I can excite ideas in my mind at pleasure."[5] I can "by an act of my will . . . form a great variety of them and raise them up in my imagination," though "these creatures of my fancy are not altogether so . . . permanent as those perceived by my senses."[6] "We perceive a continual succession of ideas [both of sense and of imagination]; some are anew excited, others are changed or totally disappear."[7] The ideas themselves are inactive, 'so that one idea cannot produce or make any alteration in another,'[8] but the changes are caused by an 'incorporal active substance or spirit.'[9]

[1] 'New Theory of Vision,' 1709, § 16; cf. 'Principles of Human Knowledge, 1710, § 43.

[2] 'New Theory,' § 25.

[3] 'Princ.,' § 30.

[4] § 33.

[5] 'Princ.,' § 28.

[6] 'Dialogues between Hylas and Philonous,' 1713, II.

[7] 'Princ.,' § 26.

[8] § 25.

[9] § 26.

The succession of one sensation to another is attributed by Berkeley to Deity;[1] while the succession of a representation to a sensation is due to habitual coëxistence; 'from a frequently perceived connection, the immediate perception of ideas by one sense suggests to the mind others, perhaps belonging to another sense, which are wont to be connected with them.'[2] The same is true of the succession of one representation to another: "In certain cases a sign may suggest its correlate as an image, in others as an effect, in others as a cause. But where there is no such relation of similitude or causality nor any necessary connection whatsoever, two things by their mere coëxistence, or two ideas merely by being perceived together, may suggest or signify one the other."[3] The modes of association of successive ideas, then, according to Berkeley, are similarity, causality,[4] and coëxistence or contiguity. Like Locke, Berkeley emphasizes the pedagogic side of association: "To the end their use be permanent and universal, these combinations must be made by rule and with wise contrivance."[5]

The fact of simultaneous association is also brought out in Berkeley's writings. "A certain color, taste, smell, figure, and consistence, having been observed to go together, are accounted a distinct thing signified by the name apple; other collections of ideas constitute a sky, a tree, a book, and the like sensible things."[6] And "men combine together several ideas apprehended by divers senses at different times or in different circumstances, but observed, however, to have some connection in nature either with respect to coëxistence or succession; all which they refer to one name and consider one thing."[7] That is, the contiguity of sensations is the basis of the simultaneous association of ideas.

Berkeley's work transformed the problem of knowledge

[1] § 30.

[2] 'Dial.,' I.

[3] 'Theory of Vision Vindicated,' 1733, § 39.

[4] 'Causality or some other necessary connection if there be such,' seems to be his meaning.

[5] 'Princ.,' § 65.

[6] 'Princ.,' § 1.

[7] 'Dial.,' III.

into a distinctively psychological one. Although his philosophical standpoint is idealistic, it rests on experience, and his analysis is more thoroughly empirical than that of Locke and Hobbes. His theory of visual space perception especially called attention to the fact that our knowledge of distance away from the eye is capable of resolution into simpler experiences; and this led his successors to go deeper into the analysis and synthesis of all classes of experience. That his idealistic metaphysics affected the trend of English thought comparatively little, while his psychological empiricism exerted so great an influence, would lead us to regard the psychological side of his theory as more important historically than the philosophical side.

VI

David Hume (1711–1776) was the first after Aristotle to attempt a thorough classification of the modes of association. Indeed, he claims for himself entire originality in this field. "Though it be too obvious to escape observation," he says, "that different ideas are connected together, I do not find that any philosopher has attempted to enumerate or class all the principles of association—a subject, however, that seems worthy of curiosity. To me there appear to be only three principles of connection among ideas, namely, *resemblance*, *contiguity in time or place*, and *cause or effect*. . . . That this enumeration is complete . . . may be difficult to prove to the satisfaction of the reader, or even to a man's own satisfaction. All we can do, in such cases, is to run over several instances, and examine carefully the principle which binds the different thoughts to each other, never stopping till we render the principle as general as possible. The more instances we examine, and the more care we employ, the more assurance shall we acquire that the enumeration which we form from the whole is complete."[1] Whether Aristotle's classification was unknown to Hume, or was considered by him a mere casual list like Berkeley's, we can not be sure.

[1] 'Enquiry Concerning Human Understanding,' 1748, § 3; *cf.* 'Treatise,' Bk. I., Pt. I., § 4.

But it seems probable that Hume was not aware of Aristotle's attempt.

A significant contribution also, though only a verbal one, is Hume's alteration of the meaning of the term *idea* in the direction already suggested by Berkeley. Hume divides experience into *impressions* and *ideas*. Impressions consist of sensations, passions, and emotions 'which enter with most force' into consciousness.[1] Ideas are 'faint images of these' (*l. c.*); they are 'copies of our impressions.'[2] Ideas are of two sorts: memories and imaginations; the former are more lively, while the latter are not restricted to the order or form of our original impressions.[3]

Having settled his terminology, Hume proceeds to consider the nature of the connection between successive ideas. "It is evident," he says, "that there is a principle of connection between the different thoughts or ideas of the mind, and that in their appearance to the memory or imagination they introduce themselves with a certain degree of method and regularity."[4] "Were ideas entirely loose and unconnected, chance alone would join them."[5] But since 'the same simple ideas . . . fall regularly into complex ones,' we must suppose 'some bond of union among them, some associative quality by which one idea naturally introduces another.'[6] "This uniting principle among ideas is not to be considered as an inseparable connection. . . . Nor yet are we to conclude that without it the mind cannot join two ideas. . . . But we are only to regard it as a gentle force, which commonly prevails."[7] It is at this point in the discussion that he enumerates his three modes of association—resemblance, contiguity, and causality.

The most interesting part of Hume's subsequent analysis for our purpose is his attempt to reduce the relation of

[1] 'Treatise on Human Nature,' 1739, Bk. I., Pt. I., § 1.

[2] 'Enquiry,' § 2.

[3] 'Treatise,' Bk. I., Pt. I., § 3. Impression as used by Hume includes what are today known as sensations and percepts; his term *idea* includes both images and ideas.

[4] 'Enq.,' § 3.

[5] 'Treatise,' *l. c.*, § 4.

[6] 'Treatise,' *l. c.*

[7] *Ibid.*

causality to an habitual experience of contiguity; for in the end Hume finds no justification for the conception of a 'necessary' relation of phenomena, such as the idea of causality ordinarily implies; the notion of a necessary connection, which Berkeley insisted upon, is based, according to Hume, not on knowledge or scientific reasoning, but on observation and experience.[1] The so-called causal connection, therefore, resolves itself into a mental relation: "reason can never show us the connection of one object with another, though aided by experience and the observation of their constant conjunction in all past instances."[2] The mind, therefore, in its passage from one idea to another, is determined not 'by reason, but by certain principles which associate together the ideas of these objects and unite them in the imagination.'[3] The belief in causation, and indeed the belief of the existence of objects, is 'a lively idea related to or associated with a present impression.'[4] The three principles which he adopts from Berkeley thus reduce to two: resemblance and (customary) contiguity in experience.

Hume makes only brief allusions to simultaneous association. Among the effects of the 'union or association of ideas,' he says in the 'Treatise,' 'there are none more remarkable than those complex ideas which are the common subjects of our thoughts and reasoning, and generally arise from some principle of union among our simple ideas.'[5] In the 'Enquiry' the reference is still more vague: "Nothing is more free than the imagination of man; and though it cannot exceed that original stock of ideas furnished by the internal and external senses, it has unlimited power of mixing, compounding, separating, and dividing these ideas."[6]

Throughout his two works Hume concerns himself particularly with the association of representative experiences. But in one place he notes that "there is an attraction or

[1] 'Treat.,' Bk. I., Pt. III., § 3.

[2] *Ibid.*, § 6.

[3] *Ibid.*

[4] Pt. III., § 7.

[5] Bk. I., Pt. I., § 4.

[6] § 5.

association among impressions as well as among ideas; though with this remarkable difference, that ideas are associated by resemblance, contiguity, and causation, and impressions only by resemblance."[1] This is a mere casual statement however, and does not belong to his general analysis, which lags behind Hobbes and Locke in this respect. He alludes to an *association of voluntary acts* in the same casual way.[2] Had he brought these concepts into vital connection with the rest of his work, his psychology would undoubtedly deserve to be classed as associationism. But it was reserved for Hartley to extend the principle of association systematically to all classes of mental phenomena.

The psychology of Hume is a rounding out of Berkeley's and Locke's. Starting, like them, from the empirical standpoint, he employs a more rigid method, especially in analyzing the notions of causality and necessary connection, and advances to the epistemological conclusion that all knowledge is knowledge of experiences, and that the relations of 'things in themselves' can never be known.

Historically, his work forms the starting-point of two widely distinct movements in philosophy and psychology. The influence of Hume transformed the dogmatic attitude of the earlier German school into a critical one; Kant and his successors were impelled thereby to seek in experience some validation of the deductions of reason. On the other hand, the British school, which had already abandoned the notion of innate ideas, sought by further analysis of consciousness in terms of association to obtain the utmost out of experience. In England, philosophy after Hume became *associationistic* as well as *psychological;* while the Scottish school, inaugurated by Thomas Reid, followed Hume in the empirical analysis of consciousness, but held that experience gives a direct, intuitive knowledge of things; hence it found less need for the principles of association. In the line of direct development, Hartley's analysis is a natural extension of Hume's; but the publication of his chief work followed

[1] 'Treat.,' Bk. II., Pt. I., § 5.

[2] *Ibid.*, Bk. II., Pt. III., § 5.

closely after Hume's 'Treatise' and was partly anticipated in an earlier article. We must therefore attribute to the influence of earlier writers much of the initiative for Hartley's elaboration of the associative laws.

VII

One other writer should be mentioned before we pass to Hartley. John Gay (1699–1745), a clergyman and cousin of the poet of like name, sought to derive the moral principles from association. His work appeared anonymously in 1731, as a prefatory essay to the English translation by Edmund Law of Archbishop William King's 'Origin of Evil'; it was entitled 'Dissertation on the Fundamental Principle of Virtue,' and was acknowledged as Gay's in the fourth edition. In this 'Dissertation' our motives to action are attributed to pleasure and pain—man 'pursues the former and avoids the latter.'[1] The aim being happiness, the course of action is regulated by the "association of ideas" of pleasure or pain[2] and these associations lead to "habits."[3] That which man 'apprehends to be apt to produce pleasure, he calls good and approves of.'[4] Good and evil acts, even in the imagination, 'have a present pleasure and pain annexed to them, proportionable to what is apprehended to follow them in real existence';[5] and certain things become 'so tied together and associated in our minds' with pleasure or pain, that one can not present itself but the other will occur; and the association remains even after that which at first gave them the connection is quite forgot.[6] Some of these associations are not original, but are learned 'by imitating those whom we admire.'[7] Gay's theory was the direct precursor of the utilitarian movements in ethics.

In 1747 appeared another short anonymous work, entitled 'An Enquiry into the Origin of the Human Appetites and

[1] § 3; p. xxii, ed. of 1731.
[2] P. xiv.
[3] *Ibid.*
[4] § 3, p. xxii.
[5] *Ibid.*
[6] § 4; pp. xxx–xxxi.
[7] P. xxxiii.

Affections, shewing how Each Arises from Association,' which has been attributed to Gay,[1] with what authority I am unable to say. The views which it advances may well have been Gay's, but the frequent references to Gay's 'Dissertation' are entirely impersonal (*e. g.*, § II., 26; VI., 22). Among the positions taken in this 'Enquiry' the following may be noticed. (1) *Inseparable union:* "The joint appearance of two or more ideas frequently in the mind is for the most part changed into a lasting and sometimes an inseparable union";[2] this is illustrated by the child's hearing the sound of the word *nurse*, and at the same time seeing the nurse.—(2) *Necessary connection:* "There seems to be a necessary correspondence or connection betwixt some ideas and others."[3] Thus the ideas of space and time 'associate themselves with and are inseparable concomitants of all others';[4] the ideas of husband and wife are a 'natural' association, etc.—(3) *Coalescence of ideas:* 'When two or more objects at the same time strike the same or different organs, if the impressions are again and again repeated the ideas they excite will coalesce and unite.'[5] The author also repeats the view given in the 'Dissertation' that some of our associations 'we learn from imitation.'[6] He calls attention to the decrease of pleasure and pain by repetition of the experience. The latter part of the 'Enquiry' is an application of association to ideal pleasure and pain and to the principles of morality.

[1] Reprinted, 1837, by Samuel Parr in the 'Metaphysical Tracts'; the latter were printed after Parr's death, and the publisher found no clue to the authorship of the 'Enquiry.'

[2] § II., 18.

[3] § II., 41.

[4] *Ibid.*

[5] § II., 41.

[6] § II., 43.

THE EFFECT OF HIGH RESISTANCE IN COMMON NERVE PATHS

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In the study of intelligent behavior we find that the field ranges from the comparatively simple to the exceedingly complex. For the simpler forms of behavior explanations in terms of nervous mechanisms are generally admitted. As we advance towards the complex, however, we find that authorities diverge more and more and also that there is, as we so advance, an increasing tendency to explain behavior in terms belonging to the subjective aspect, as it were. From one point of view this tendency is a decided obstacle to general progress. In the study and discussion of behavior showing a high degree of intelligence, if we can formulate rational theories of nervous mechanisms, it will help us break away from the methods of subjective psychology.

There are learned men who believe that the key to an understanding of mental growth will be found in the changes that occur in the relative conductivity of the nervous fibers that connect the receptors and effectors of the body. These fibers are to be viewed as competing pathways.

In this article we shall have to consider cases where the competing paths are many in number as well as those in which they are few. It is our purpose to discuss some suggestions as to the effect of local resistance in such nerve paths upon the control of discharges, and thus indicate the nature of certain nervous mechanisms.

To begin with let us briefly consider how nervous pathways are constituted. Each path is made up of two or more neurones (nerve cells) in relays like a linked chain. Each link of the chain is a neurone and the discharge of each neurone excites the next in relay. A single neurone, however, on the one hand may have several tributary neurones, or on the

other hand it may be tributary to several neurones. The junction of each neurone with the next is known as a synapse. Just how the impulse is conducted through the chain of neurones is not well understood.

One writer of a book on psychology states:[1] "The course of an impulse through the cortex is largely controlled by synapses that owe their degree of resistance to the frequency with which they have been used. . . . The change that comes with action is a lessened resistance at the synapse. . . . The formation of habits is thus a process of decreasing the resistance of the synapses in the different possible paths of transmission."

To fit with these ideas we must allow that the impulse is divided among the possible paths, a large discharge going where the synapse is open and a small discharge where it is not open. This must be so, for it is by repeated small discharges that the resistance is decreased.

In our demonstration we may assume that an impulse leaving a receptor has several different possible paths of transmission. At some point in each of these paths we may assume there will be a nerve junction of relatively high resistance. The rivalry between these resistance points is what determines which path will be taken by the larger impulse at any given time. We may assume also that each resistance point has a resistance that varies with the frequency and recency of prior discharges. For convenience in discussion, let us term such a point of high resistance an outpost junction. Let us fix it in our minds that the impulse travels along its several paths, encountering very little resistance until it reaches the outpost junctions. It then selects the junction offering the lowest resistance at the time and the main discharge follows the course thus chosen to some motor or effector terminal.

We see that a given receptor might have a large number of outpost junctions. On the other hand, we see that for a given outpost junction there may be other (private) paths coming from other receptors. Suppose then that an impulse reaches

[1] Pillsbury, W. B., 'Essentials of Psychology,' New York, 1912, pp. 50–55.

an outpost junction coming from the first receptor which we will call *S A* and finds the resistance low. The impulse will pass on to a certain effector *M A*. Suppose also that another time an impulse comes from a second receptor *S S* to the same outpost junction and finds the resistance low. Again the impulse will pass on to *M A* and the result at the effector will be just the same as if *S A* had been excited. In other words this outpost junction lies on a path that is common to the two receptors *S A* and *S S*. Furthermore from our premises this outpost junction controls the common path in question. It is plain that every time there is excitation at *S A* or *S S* there will be some discharge that will reach *M A* and that the discharge will be great or small depending upon the openness of the junction compared with the openness of rival outpost junctions, so to speak. It is also clear that the openness or lowness of resistance at this particular outpost junction, which we will call *X*, is dependent upon the number and recency of excitations at *S A* and *S S*.

Let us now suppose that *S S* commands a hundred outpost junctions and that at first they are of equally high resistance. Now if there be an excitation of *S A* followed shortly by one of *S S*, the largest discharge from the latter will be through *X* to *M A*, because that junction has just been opened by the impulse from *S A*. So we have here a mechanism by which the excitation of one receptor influences the response from another receptor. It is a very simple mechanism, but it is thought to be adequate to account for the selective distribution of nervous impulses.

The outpost junction may be located at a synapse of some connecting neurone. The connecting neurone which is commanded by both receptors and which ends in junction *X* may be termed an association neurone. The reader may prefer to think of the association neurone rather than the outpost junction as the determining factor in associative memory.

If every one of the outpost junctions commanded by receptor *S S* is also connected with one or more other re-

ceptors, we may have a joint mechanism such that the excitation of S S will produce a response in any one of a hundred different effectors if the right receptor has been more recently or frequently excited. The function of this joint mechanism may be termed the selective distribution of nervous impulses.

When we say that the meaning of a certain word has to be judged by the context, we might better express the case by saying that the response to the excitation of a certain group of receptors depends upon what other receptors have been lately stimulated. The proper selective mechanism, of course, must be there.

Let us fix it in our minds that an important feature of these mechanisms is that there is low resistance from the receptor to some point in a common path and at this point there is high resistance. This relatively high resistance in the common path compared to the resistance of the private path is, so to speak, an essential factor.

It should, of course, be remembered that such a unit mechanism as we have outlined does not in reality act alone but always in concert with other unit mechanisms. The least change in the environment means the excitation of many sensory neurones and the resultant nervous discharge to the effectors determines the response.

As an illustration of the functions of these selective mechanisms, let us think of a trained animal. The setting of the stage supplies a series of impulses that arouse certain association neurones. When the trainer gives the signal the animal performs his act. When the stage setting is changed, he will at the signal do a different trick, because other association neurones have been aroused. In each case the sight of the stage properties excites impulses that arouse a certain set of association neurones and therefore open a certain set of synapses commanding nervous pathways appropriate to the performance.

In the mechanisms that we have been considering the rule is that the impulse from one private path opens the way for an effective impulse from another private path. Let us

now inquire into mechanisms that have a similar structure but a somewhat different function. If two receptors have a common path of such degree of resistance that at first the discharge through it is ineffective, but after each time that the receptors are excited consecutively the resistance of the controlling synapse is greatly lowered, a useful structure is provided. It is evident that the path will become open for an effective discharge from either receptor. If the path leads to an effector only reached in the first place by effective impulses coming from the first receptor which we will call *S* 1, while afterwards it is reached also by effective impulses from the other one which we will call *S* 2, the latter receptor is given new power. On further consideration we see that the arrangement provides for associative memory.

It must be noted, however, that a unit mechanism of this sort should have only two tributary receptors, so that the development of the mechanism depends strictly upon the excitation of the two in conjunction. In this way we get a true correspondence of the nervous development with the environment. The pathway must be common to two private paths only.

The functions of nervous mechanisms of this class depend upon the system of effector connections, as it were. With an appropriate system, the mechanism will provide for substitution as it is called, *i. e.*, where one signal comes to take the place of another in provoking a given response.

We may illustrate this case by the example of an animal that runs to the feed box when it hears the keeper coming with feed. An auditory signal has become substituted for the visual signal that is to follow.

On consideration we now see that a synapse mechanism of the type described herein will serve in one case for the selective distribution of impulses and in the other case for the linking of one impression with another in the formation of habits. We recall that the essential features of such a mechanism are low resistance in the private path coming from the receptor and relatively high resistance in the synapse commanding the common path. In other words, local re-

sistance in nerve paths will serve to control nervous discharge, and a useful arrangement of connecting nerve paths with appropriate points of resistance constitutes a nerve mechanism.

It is clear that by the concerted action of such mechanisms is made intelligent behavior, *i. e.*, behavior determined by individual experience.

DISCUSSION

ON COLOR THEORIES AND CHROMATIC SENSATIONS

BY CHRISTINE LADD-FRANKLIN

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A recent issue of the Cambridge Psychological Library—a book on 'Color Vision' by Dr. Parsons—is so good in itself and so well adapted to bringing to the fore the crucial difficulties of the Helmholtz and of the Hering color hypotheses that it invites extended discussion. It is not to the physicist and to the follower of Helmholtz that the book is especially calculated to be beneficial; they —alas!— will be too apt to be confirmed in the present error of their ways, for the psychological point of view, which demands explanation of the yellowness of yellow and of the whiteness of white, is a point of view which Dr. Parsons attains to only fleetingly and superficially. Nevertheless, the space which is given to the work of Hering, and the recognition of the widespread acceptance accorded to his views, will perhaps have a subconscious effect in laboratories where his name and his splendid fight for the psychology of color are practically unknown. It is to the adherents of the other school —the followers of Hering—that the book is destined to be invaluable. It is an attractive piece of book-making—a fact of no little importance; it is freely provided with diagrams—which act as *aide-memoires* far better than columns of figures, and which are not, in this instance, like the diagrams of Hering, free-hand drawings based purely upon the imagination; and it offers a thoroughly well-informed and acutely criticized summing up of the facts regarding color-sensation, especially those facts which have their origin in the laboratories where Helmholtz is still followed, and which are therefore *res non gratæ* to too many of the psychologists. Hereafter (on account especially of the evident effort at fairness of this book) it will not be possible for the facts which plainly contradict it to be utterly ignored in the schools where the Hering theory is made a matter of religious faith. (I am personally particularly glad to welcome so strong a defender of some of the doctrines which I have long been preaching.)

In order to obtain clearness of vision in the present situation as it regards the visual sensations, it is necessary to keep constantly in mind the sharp distinction between the Helmholtz *theory* and the Helmholtz *facts*—the facts which are supposed to support it. The Helmholtz theory has very properly long ceased to be an existent thing in the mind of the psychologist, but he has too often made the mistake of throwing away the baby with the bath,—the mass of facts which have issued from the Helmholtz laboratories are held by him in as bad odor as the theory itself. These facts group themselves, in good part, about the so-called 'color'-triangle, and the extent to which it has been of recent years possible to ignore them is evidenced by the circumstance that the very existence of the color-triangle has been, in certain laboratories, practically forgotten. But this is a great mistake; there is no necessity for ignoring these facts; it is perfectly possible to conceive of them as compatible with the great discovery of Hering (re-discovery rather—the fact was never doubted from the time of Leonardo da Vinci until the coming in of the Young-Helmholtz fallacy), namely, the tetrachromatism of the visual sensations and the independence of the sensation of

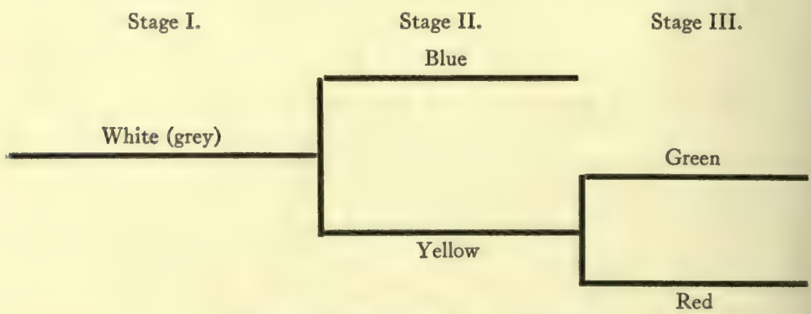

FIG. 1. Development of the Color-Sense.

whiteness. It was, of course, expressly for the sake of taking account of *both* these sets of facts, supposed by their respective defenders to be irreconcilable with each other, that I was forced, some time ago, to devise a different hypothesis—a conception which not only takes them both in, but which also accounts, at one blow, for the fundamental mystery of vision—the vanishing of the opposite color-pairs—and for the fact that the development of the color-sense is by way of a middle stage of blue and *yellow* (the poor unowned color of the physicist) (Fig. 1). In other words, this theory, the Development Theory, harmonizes and unifies, by means of a single,

simple hypothesis, these four things: the 'color'-triangle (what should be called the three-stimuli triangle or the three-receptor triangle—Fig. 2), tetrachromatism, complementation, and color development. My theory, in fact, succeeds in being, like that of Helmholtz, a three-stimuli theory, at the same time that it is, like that of Hering, a theory of tetrachromatism. The main value of Dr. Parsons's book lies in its forcible presentation of the three-stimuli view (which the followers of Hering are obliged to absolutely ignore), but he has not got psychological considerations sufficiently in the blood to perceive how indispensable it is to adhere to a theory which *unites* with that view the fact of tetrachromatism. I have discussed this aspect of my theory elsewhere.[1] That Dr. Parsons feels the necessity of having some adequate theory in terms of which to express the vastly complicated facts of vision is evidenced by his devoting the last hundred pages of his book—a little more than a third of it—to the explicit discussion of color theory (what ought rather to be called color hypothesis). It is, indeed, a disgrace that the two rival older theories should still have each its ardent defenders, who are incapable of seeing that the views which they uphold, respectively, have been again and again wholly annihilated by the party of the other part. They may be said to parallel the two forms of color blindness, each incomprehensible to the other, where one set of defectives is able totally to ignore the existence of red and green, the other the existence of blue and yellow. But it is to be hoped that this book of Dr. Parsons will have the effect of forcing each party to recognize the existence of the other,—of bringing the rivals to grips. Certainly no college professor, whether in physics, in physiology or in psychology, can hereafter lecture to his students on color without being aware that this fatal volume may easily be in their hands.

Dr. Parsons professes to give in the first and second parts of his book an account of facts only, and to treat theoretical considerations (together with such facts as are closely bound up with theory) only in Part III. This is, of course, an illusion. The two theories which he has chiefly in mind have each its own language, and no one can write, or speak, for five minutes on the subject of color without giving away that he does or does not accept certain of the fundamental assumptions of one or of the other. Thus the view of Hering that the subjective intensity of, say, a whitish bluish green is due solely

[1] See *Mind*, 1892; 'Dict. of Phil. and Psychol.,' Art. *Vision;* 'Am. Encyc. of Ophthalm.,' 1913, Art. *Color Theory*, etc.

to the subjective intensity of its whiteness component, either is or is not a part of the speaker's mental furniture. This curious view of Hering's is, of course, a consequence of his having adopted the belief that the vanishing chroma-pairs, say yellow and blue, are antagonistic instead of white-constitutive. The chromatic sensations certainly vanish, and white appears in their stead: that they vanish may, in the first instance, just as well be supposed to be due to their having annihilated each other (as for instance an oxidation and a reduction would do) as to anything else. But whence comes the substratum of the sensation of whiteness which takes their place? Either it is a residual matter, left behind, and present already in the blue and the yellow separately, or it is something which is constituted out of the processes of the blue and the yellow (just as, if there happened to be present at once an acid and a base, a salt would be formed and the acid and the base would both disappear). The destruction view looks good enough, at first sight, for yellow and blue—already it excites question for red and green, because those colors are not white-constitutive but yellow-constitutive. (It is for this reason that the physicist—a lover of truth—has never been able to give a moment's consideration to the theory of Hering.) But the strain upon one's powers of belief when one is asked to think that all the intensity of say a wholly saturated red is due to the intensity of its whiteness component, when there is no reason to suppose that it has any whiteness component whatever, is certainly very great. Dr. Parsons is of course not capable of this logical *tour de force*, and it is only the fact that he cannot really, even for a moment, think himself out of the common sense view that brightness is brightness, that enables him to imagine that he is not committed against all the vagaries of Hering from the beginning. In spite of this practically unavoidable illusion, however, he has succeeded in giving to his book a very commendable aspect of fairness.

Dr. Parsons's work exemplifies in a striking degree what I have elsewhere insisted upon—that the hopeless *impasse* that exists regarding color would be in a great measure obviated if we were to adopt a more accurate terminology.[1] What I have proposed is, in part, that the term *color*, now hopelessly ambiguous, should be used rigidly in that one of its two present senses in which it *includes* the achromatic sensations, and that for the sensation of color proper we should use the term chromatic sensation, or, simply,

[1] *Psychol. Bulletin*, February, 1913, abstract of paper on 'Color Terminology.'

chroma. (We should then have the terms chromaticity and achromaticity for 'degree of saturation' and 'degree of non-saturation' respectively.) Toned and toneless colors (the Hering terms) are also perfectly correct.—Physical light rays should not be said to be red, green, etc., but to be erythrogenic, chlorogenic, leucogenic, etc. They get their color only *after* they have been passed through the retinal receiving station.—Instead of 'brightness' (made wholly ambiguous by Hering's use of it) *subjective intensity* should, at least at present, be used, or even, for the sake of freshness, the term of Hess, *lamprosity*.

As regards the term *color*, since language affords us the two terms uni-color and monochromatic—color and chroma—of Latin and of Greek origin, why should we not at once particularize their meaning? To *préciser* the meaning of superfluous words is to secure progress in accuracy of speech, and in this case nothing could be simpler than to use *color* regularly in its extended meaning, and *chroma* for the specific meaning, color proper (as is done in real life). There is no *color* theory which does not include a discussion of the achromatic sensations. Is it not an absurdity to say, in the present loose meaning of the terms (as is constantly done in this book), that the sensations of the totally *color-blind* are *monochromatic?* The light sensations of the defective in question (and of the normal individual under various special conditions) are of one *color* only, it is true—the quality of the spectrum is white, in such cases, throughout its whole extent; but they are not monochromatic—no chromatic quality is perceived at all, but only an achromatic one.

But the real seriousness of a defective terminology occurs in connection with the discussion of the facts which are represented diagrammatically in the so-called 'color'-triangle. These facts and their significance are given their proper importance in this book, but they would be far less open to misconception if the language in which they are expressed were more carefully guarded.—To mention, preliminarily, a minor criticism in connection with this topic, it is a pity that the two triangles which are represented in diagrams (pp. 39, 40) are differently drawn about an axis of symmetry—this obscures their resemblance, to the cursory reader. In fact, the color triangle, no matter how it is laid down by the original experimenter, ought always to be drawn about the yellow-blue line as an axis of symmetry (Fig. 2), in this way is made patent to the eye the uniqueness of the *R Y G* side of the triangle, the fundamental character of yellow-blue vision, and also the peculiar mode of development of

the color-sense,—the primitive character of the color system: yellow, whitish yellow, white, bluish white, blue. Thus on the line *Y W B* are represented *all* the yellow-white-blue sensations of the red-green blind of either type; and the "confusion" colors (so

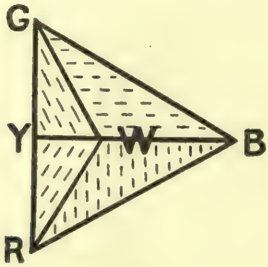

FIG. 2, *a*.

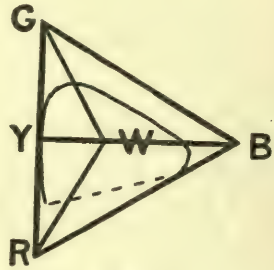

FIG. 2, *b*.

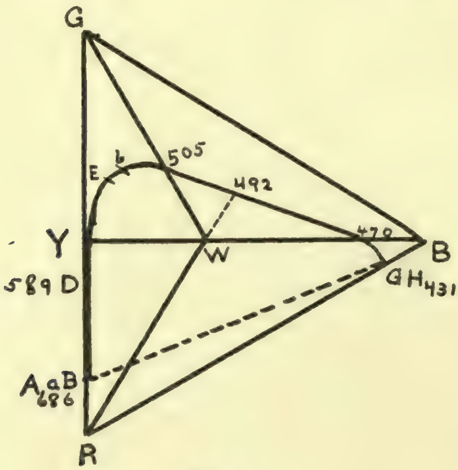

FIG. 2, *c*.

FIG. 2. The So-called "Color-Triangle," quadrigeminal in character to represent tetrachromatism, but triangular in shape as expressing the facts of matching by light-ray mixtures. The chromatogenic radiations are here represented, in trilinear coöordinates, as functions of three independent variables.

mysterious to those who bother with the Holmgren wools) are one and all depicted upon lines which pass through the points *R* and *G* respectively and cross the line *Y B*.[1] Hence the convenience of making the line *Y W B* a conspicuous line in the figure is apparent.

The pivotal statement in Dr. Parsons's book, about which his

[1] See 'Diction. of Philos. and Psychol.,' article *Vision*.

gument mainly centers, occurs when he says, speaking of certain lucubrations of Edridge-Green, that they are an inadequate explanation 'of the trichromatism of normal vision, *which is a fact, not a theory*.' (Italics mine.) Now it is perfectly true that what Dr. Parsons here intends to refer to is a fact and not a theory; but it is not trichromatism, and it will never be generally accepted as a fact so long as it is called by that name. 'Trichromatism' is nonsense. Every psychologist knows (and many a physiologist, but no physicist) that vision is tetrachromatic, that there are four distinct chromatic sensations, Red, Yellow, Green and Blue. (I adopt the use of capital letters to indicate the exact, simple, unitary, chroma-sensations, while red (*e. g.*) with a small letter, may still be used loosely, as in real life, for a sensation which, though it may be slightly bluish or yellowish, a little 'off color,' still has redness for its predominant quality.) The fact which Dr. Parsons is here referring to is (if expressed in the terms demanded by our present knowledge of photochemistry) that the resonance curves which represent the response of the receptor substances of the visual apparatus to light are three in number, that the activity of the receptor apparatus can be expressed as a function of three variables, and that the area which gives point-to-point representation to distinct functions of stimuli is triangular in shape. In Fig. 2, *a*, the triangular character of light-ray mixtures is represented roughly; in Fig. 2, *b*, the exact spectral curve is reproduced. While the shape of this figure is triangular, as representing the facts of 'matching by mixtures,' that the result is tetrachromatic may be very simply but quite adequately represented by the four-fold character of the striations (or by actual pigments). I have called this the quadrigeminal (but triangular) color area,—it unites in one diagram the beloved color facts of Hering and of Helmholtz. (Quadrigeminal is reminiscent of the *corpora quadrigemina*.)

The tridimensional figure, of which this is a cross-section, is usually given as a double pyramid, but that is a total misrepresentation of the real state of things. There is no reason why the accompanying achromatic process and sensation (present, as is now well known, in the cones also together with the chromatic one) should not be represented. The correct diagram for this has been given in longitudinal section by Wundt,[1] and it is so important that I reproduce it here (Fig. 3) in the hope that it may come to take the place, in text-books, of the erroneous double pyramid. After

[1] 'Phys. Psychol.,' 4. Aufl., S. 537.

objective light has reached a certain (low) intensity an achromatic (prechromatic) sensation sets in, and increases from *b* to *w*. The chromatic constituent of the total sensation begins at *c* and takes the course *c r*. The dotted line indicates the chromaticity of the complex sensation,—it reaches a maximum at *m*, and then approaches zero. Wundt has represented the chromaticity as a difference,—it should, of course, be a ratio, but the principle is the same. A cross-section at any point of the solid of which this is the longitudinal section would give the color triangle at a given intensity.

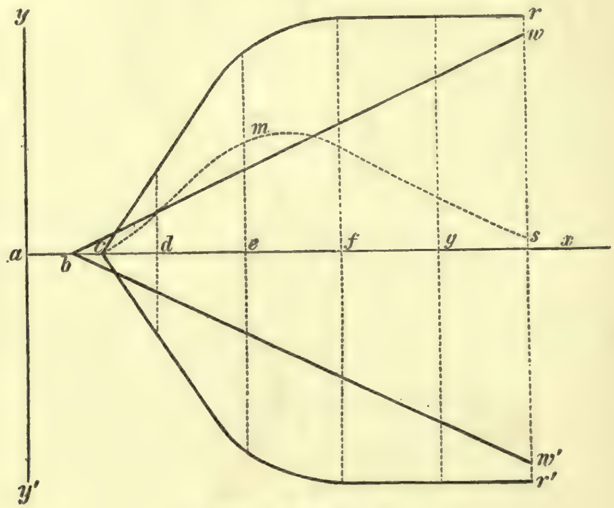

FIG. 3. See text, p. 243.

Exactly the same state of things as is depicted in the color triangle is given again, by another method of representation, in the three resonance curves of König (Fig. 4). The fact that Dr. Parsons has searched well the literature of color is evidenced by his giving these curves in their latest form—that required by the fresh determination of the intersecting points (the points where change of objective intensity causes no change of color tone) by F. Exner.[1] These curves differ from the similar, quadruple, curves of Hering, of course, in the circumstance that, while the latter are the pure work of the imagination, these are the result of the most exact measurements. This diagram ought to be adopted as the classical representation of these facts. Miss Steindler's determination of

[1] *Sitz. d. Wiener Akad.*, 1902, CXI., iia, 857.

the four points of maximum discriminibility for color tone (page 32) is also of critical importance.

It is on the side *R G* of the Helmholtz color-triangle (Fig. 2) that is exhibited the character of color vision which makes the Helmholtz color theory impossible: while on the other two sides light-ray mixtures are simply correlated with sensational color blends (the blue-greens and the blue-reds), as the theory requires, on the side *R G* what should be the red-greens are replaced by blends with an entirely new sensation, yellow. This has to be accounted for *without giving up* (as Hering does) the absolutely established laws of light-ray mixture, and the fact that the sensation systems of the dichromates are reduction-forms from those of the normally visioned. It is here, therefore, that it is absolutely necessary to

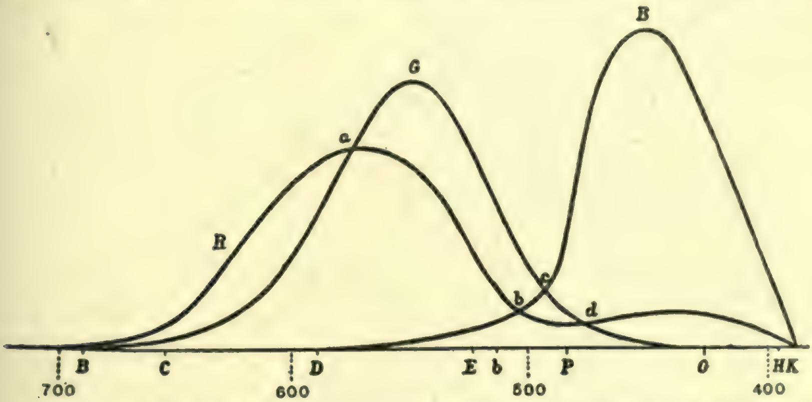

FIG. 4. *R*, *G*, and *B*, resonance curves. These are the curves of König and Dieterici corrected to new determinations of the points of section, *a*, *b*, *c*, *d*. Abscissæ, wave-lengths of the interference spectrum of the arc light; ordinates, arbitrary scale. (F. Exner.)

distinguish sharply between the visual stimuli, with their dissociation processes in the receptor substances of the retina, and the resulting nerve-excitations (with their attached sensations). But the curious thing is that Dr. Parsons himself is, upon occasion, perfectly aware of the necessity for noting this distinction. He knows well, and he explicitly states it in one good passage, that the specific light radiations are not yet chromas, but that a chroma is the conjoint effect of a specific, correlated, resonating, photo-chemical substance in the retina—the receptor mechanism—together with a nerve-end excitation and a nerve-fiber conduction, doubtless specific also, and excitable cortical (or sub-cortical) nerve cells. He

says that it is only in a 'broad' sense that you can speak of objects, or of radiations, or radiation-groups, as being colors,—he should say that it is only in a popular and wholly inaccurate sense that you can speak of them as such; they are really nothing but red-producing, chromatogenic, erythrogenic, xanthogenic, etc., objects and rays. The confusion which is here so fatal we owe, of course, to our primitive ancestor, the naïve realist. The situation is, however, equally fatal to every form of realism, as has often been pointed out, and brilliantly by Professor Lovejoy. In most of our interests, both scientific and non-scientific, this ambiguity does not trouble us, but occasions may at any moment arise when (as in the present instance) it is of very great import. One should, therefore, even if one does not at once use it for every day, have an adequate terminology at hand for those occasions when to distinguish between sensation and the physical correlate of sensation is an absolute necessity.

The very discreditable state of color discussion which has been kept up for fifty years may then be summed up in a word in this way. The theses maintained by the adherents of the two rival schools are these:

Helmholtz	*Hering*
Trichromatism is a fact.	Tetrachromatism is a fact.

These are evidently two absolutely contradictory statements, but both true. *Que faire?* At this point I felt myself obliged to interfere, with (1) a reformed terminology, and (2) an adequate color theory. I substitute for the above two statements this:

The Development Theory

> Tri-receptorism is a fact and tetrachromatism is a fact, and these two facts are reconciled in the development color theory,—indeed, they constitute its ground work.

To reproduce, in a word, my color theory (for which Fig. 5 offers mnemonic support): a light sensitive substance in the visual elements (the rods) in primitive times, when vapor-laden skies and phanerogamic plants afforded no chromas to be seen, responded indifferently to all parts of the spectrum; *W*, the ionic mass thrown off (photochemical reactions are now often supposed to be a form of ionization), became a nerve-end excitant to which was attached in the cortex the sensation of whiteness. In more highly developed animals—as in the bees (v. Frisch)—this cleavage product became more highly differentiated, and was capable of responding specific-

ally to the warm end and the cold end of the spectrum, furnishing respectively the sensations yellow and blue; but when both of these cleavage products are dissociated out at once, they reunite to form the whiteness nerve-excitant, *W*, out of which they were formed, after the analogy, for instance, of an acid and a base, which, being the constituents of a neutral salt, cannot (except in solution) exist separately. This form of vision persists in the partially color blind and in the mid-periphery of the normal eye. A farther specialization of response to light-rays provides 'red' nerve-excitants and 'green' nerve-excitants, but, as before, red-greenness fuses into the yellowness *out of which it was developed*, while the blue-greens and the blue-reds persist as simple chroma-blends. An exact chemical analogy for this situation can be given.[1]

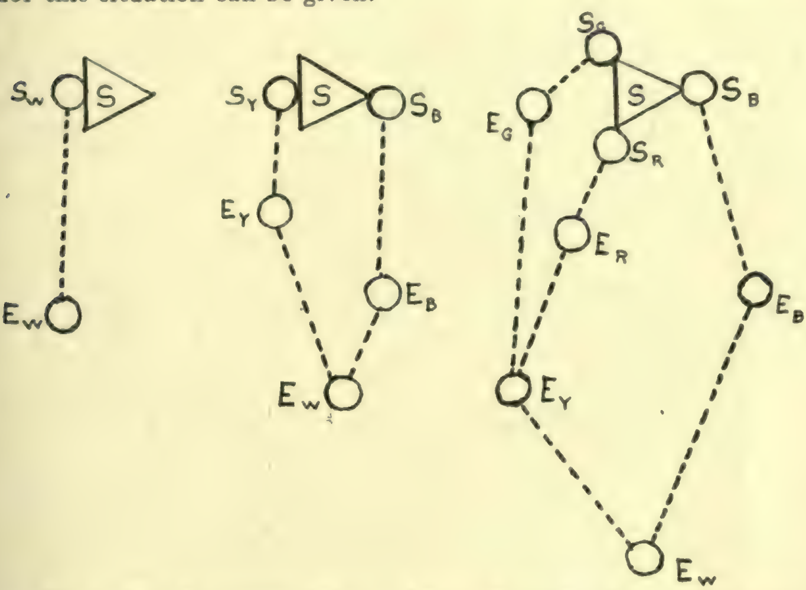

Fig. 5. The Development Theory of Color (Ladd-Franklin). Sn, the color-sensation receptors (resonators, side-chains, light-sensitive ions, or whatever the current photo-chemical theory may demand), in three successive stages of development. En, the five specific nerve-excitant substances for the five specific light-sensations.

It will be seen that my theory is built about the evident consideration that the lack of the power to perceive the yellowish blues and the reddish greens of nature—the extinction of yellow-blue and of red-green into white and yellow respectively—is a *defect* in the

[1] See 'Am. Encyc. of Ophth.,' Art. *Color Theory.*

visual mechanism,—a *défaut de ses qualités.* The best thing that could be done, given the problem of developing light-sensitive substances in the way of making wave-lengths discriminable, has been done, subject to the restriction that the whole process must be carried out in a single visual element; physically adjacent cones (unlike the mechanism for auditory specificity) needed to be utilized for a highly specific space-sense. Any color theory therefore (Preyer, Bernstein, Troland) which devises a special mechanism for the *express purpose* of making us blind to the yellow-blues and the red-greens is by that circumstance *von vorn hinein* condemned.

Some criticisms which Dr. Parsons makes of my color hypothesis may be replied to briefly as follows:

1. Nowhere in his book does Dr. Parsons make a more thoroughgoing mixing up of fact and theory than when he says that my theory is an 'offspring' of the Helmholtz *theory.* My theory takes in the Helmholtz three-stimuli *facts*—why should it not, when it was expressly devised for the purpose of bringing under a single, simple, physiological conception both those facts and the facts of tetrachromatism?—but it is as far removed as possible from the Helmholtz *theory*, which is incompatible with tetrachromatism. It might just as well be said to be an offspring of the Hering theory because it takes in the Hering facts. But every theory which deserves the name must, since Hering wrote, take in *both* the fundamental facts which are made much of by Hering and those which are made much of by Helmholtz.

2. The connection between my theory and that of Donders I have already discussed.[1] As I have just said, no theory can be proposed (since Hering's) which fails to take in both the three-stimuli view and the tetrachromatic one. Donders's theory is much more like Hering's than like mine, for a fundamental point of mine is the unique character which it assumes for yellow. Yellow is beyond question, of course, a unitary chromatic sensation, but it is far from being in all respects the same sort of thing as the other unitary chromas, any more than it is the same sort of thing as the product of the other light mixtures, the green-blue and the blue-red chroma-blends. For one thing, there are occasions (McDougall) when red and green light stimuli effects do not fuse completely, but give rapidly alternating, or possibly simultaneous, red and green sensations. In my theory the fusion of the red and the green nerve-excitants is a secondary phenomenon, and there is no reason why, under certain conditions, it should not fail to take place,—exactly

[1] *Johns Hopkins University Circular*, 1893.

(to return to my analogy) as a salt in solution is now regarded as being always partly in a state of dissociation.

3. Hering's explanation of simultaneous contrast is purely a verbal explanation. Since it has become known (Tschermak, Fernald) that simultaneous contrast is produced in supraretinal regions (also, at least, if not only), satisfactory explanations of it would be hard to discover. No one states more forcibly than Dr. Parsons that theoretical explanations in regions where facts have not yet been completely made out are waste of time. Simultaneous contrast *over wide regions* is doubtless an electrical phenomenon of some sort, attendant upon the main events of nerve fiber excitation (as suggested by Troland). Tashiro's brilliant discovery of the increased giving off of CO_2 during the passage of a nerve current may be suggestive in this connection, and so may the subjective vision of the nerve current in the nerve fibers of the retina—that remarkable fact which has had six independent discoverers, Purkinje the first, and which is still nevertheless almost unknown.

4. Dr. Parsons suggests the addition of a core of undifferentiated atoms to my assumed light-sensitive (resonating) molecule. One thinks of selective dissociation by light now, of course, in terms of side-chains or electrons (or probably, by this time in terms of magnetons) attached to a residual substance. But even at the time I wrote my first article on this subject my assumed molecule had already a 'core' for the support of its resonating attachments, as Dr. Parsons would see if he were to look at my original diagram, and in fact I expressly mention its importance.[1] But the real answer to the supposed objection here intended (an objection which shows complete failure to understand the theory) is that (to repeat my analogy) if a salt were added to something in a test tube the effect would be the same as if there were added to it the acid and the base out of which the salt is constituted. It is, in fact, the theory of v. Kries (not mine) which is made impossible by the circumstance that there is no reason why his foveal white should look like his rod white, since their supposed physiological substrates are entirely different; my criticisms of his views on this point have been explicitly accepted by Professor Hering.[2]

Dr. Parsons's book, however, in spite of fundamental points in which I maintain that it is open to criticism, can be safely recommended as indispensable in any place where the science of color-sensation is under serious consideration.

[1] *Zsch. f. Psychol.*, 4, page 6.

[2] *Pflüger's Archiv*, 1907.

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A RECONSIDERATION OF JAMES'S THEORY OF EMOTION IN THE LIGHT OF RECENT CRITICISMS

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When Professor James first published his theory of emotion in 1884 he was subjected to a storm of ridicule such as has fallen to the lot of few psychologists. This both the theory and the author survived, and by his statement in the 'Principles,' but especially by the Psychological Review article of 1894, he succeeded in pacifying most of his critics. For a number of years the revised doctrine enjoyed an ascendency but feebly disputed among English-speaking authorities. In 1900, however, came Sherrington's striking experiments dealing with the results of spinal transection in the dog, and James's theory once more fell under a shadow, this time apparently a more menacing shadow because cast by the solid wall of carefully observed fact. Now comes our fellow countryman, Dr. Cannon, with further considerations based on physiological experiments tending also to discredit views of the Jamesian type, although not in so many words attacking James's own formulation. Where the expert speaks cautiously and generalizes conservatively, the amateur and the dilettante rush boldly in and scatter sweeping indictments broadcast. Thus it has already come to pass that the critical queries of these eminent leaders among the physiologists of our time have been seized upon and exploited as sounding the knell of James's theory.[1] Unless some obstacle be thrown in the path, this

[1] Even careful writers like Ladd and Woodworth ('Elements of Physiological Psychology,' 1911, p. 525) allow themselves to use expressions which are certainly

movement will rapidly gain momentum and presently we shall hear that James's theory is forever disproved. The writer cherishes no illusions as to his power to furnish single-handed the obstruction necessary to slacken the pace of this tendency. But it is at least worth while to call attention to the present situation if, as he believes, the essential features of Professor James's view are profoundly true, and as yet quite untouched by any evidence, whether physiological or psychological, thus far established. It will serve to throw the issues into somewhat truer perspective if we devote a few moments to the earlier and more purely psychological criticisms to which James's formulation was subjected.

Unquestionably in the first article in *Mind* (Volume 9, 1884), James expressed himself in a manner to invite an unlimited amount of needless criticism. He said, for example (p. 189), "My thesis . . . is that *the bodily changes follow directly the perception of the exciting fact, and that our feeling of the same changes as they occur* is *the emotion.*" Instantly the pack was after him in full cry. What! a description of emotion with no mention of pleasure-pain? An account too that took no notice of the cognitive features essential to every reputable emotion? What of the purely spiritual element so subtle and yet so obvious and so priceless in every esthetic and religious emotion? Moreover, was it not clear that many of the deepest and most pungent of emotions (*e. g.*, dry-eyed grief) were characterized by little or no bodily disturbance, a result wholly inexplicable in terms of James's theory? Most crushing of all, certain clinical cases of general anesthesia were put forward as disproving James's view because, despite the anesthesia, there

loose: ". . . it [the James-Lange-Sergi type of theory] seems to have been distinctly discredited by experimentation upon the lower animals." "Sherrington by 'appropriate spinal and vagal transection' removed from dogs, completely, all sensations of the viscera, and of the skin and muscles behind the shoulder." Although the point is of no great consequence for the general issue, the 'completely' above quoted is obviously at variance with Sherrington's own statement contained at a later point in this paper, and the 'distinctly discredited' is a decidedly stronger statement than Sherrington allows himself so far as concerns James's view, which he properly distinguished sharply from those of Lange and Sergi. Nor does it seem quite fair to characterize the reduction of emotion to a vasomotor basis, which marks the contention of Lange in opposition to James's view, a 'minor difference.'

were in the patients traces at least of certain basic emotions such as fear and anger and joy.[1]

Some of these critics, as has been already intimated, were mollified though not convinced by the elaboration of the theory in the 'Principles' (1890). Most of them were measurably content with the 1894 PSYCHOLOGICAL REVIEW article, but they stoutly maintained that this was in substance a recantation of heresy, a subscription to the true faith.[2] In part it was such. It modified such portions of the original statement as had led to gratuitous misunderstanding and it made clear that the positive and cardinal part of the doctrine had been left wholly unscathed by any of the attacks. A few irreconcilables of course remained. Some persons, as James himself ruefully remarks, can never be made to understand what the theory is. For some systems too, those of Wundt and his immediate disciples, for example, the Jamesian position is inevitably indigestible and unassimilable. But broadly speaking there has been very general assent to many of its features—certainly much wider assent than to any other single formulation. Professor Dewey,[3] apparently unconsciously following a lead earlier indicated by Paulhan in his insistence on the inhibitive features of emotion,[4] has taken the more significant aspects of the theory, attached them somewhat more explicitly than did James himself to certain Darwinian principles and re-edited the whole in a brilliant formulation which has suffered even more than James's by the inability of the public to understand it. So we come to the work of Sherrington and Cannon.

Sherrington's first experiments[5] were on five young dogs in which the spinal cord was severed in the lower cervical region. Thus, as the investigator states, sundering "from the brain all nexus with the thoracic, abdominal and pelvic viscera, except that existing through certain cranial nerves. It also cuts off all the blood vessels from the bulbar vasomotor center, except

[1] Compare D. Irons, *Mind*, N.S., 1894, 3, 77; *Philosophical Review*, 1897, 6, 242, 471, 626. W. L. Worcester, *Monist*, 1892–93, 3, 285.

[2] J. M. Baldwin, PSYCHOLOGICAL REVIEW, 1894, 1, 610.

[3] PSYCHOLOGICAL REVIEW, 1894, 1, 553; 1895, 2, 13.

[4] 'Les phénomènes affectifs et les lois de leur apparition.'

[5] *Proceedings of the Royal Society*, London, 1900, 66, 390.

for certain scanty communications through the cranial nerves. The skin and motor organs are, as far as the shoulder, likewise cut off from all communication with the brain. Therefore behind that level they are precluded from contributing to nervous processes of emotion either in their centripetal or their centrifugal phases. . . . If reliance be placed on the signs that are usually taken to signify pleasure, anger, fear, disgust, then these animals showed them as unmistakably after as prior to the transection of the cervical spinal cord. . . . Toward friends and enemies among their fellow inmates of the animal house, they displayed as markedly as ever their liking or their rage. . . . I saw fear notably displayed by one of the dogs. . . . The lowering of the head, the dejected half-averted face, and the drooped ears contributed to indicate the existence of an emotion as lively as the animal had ever shown us before the spinal operation had been made." The author then cites a number of other observations on these animals, all, however, of the same order. Further experiments are also reported on other animals with variations of technique. For example, the vagi were severed in certain cases, thus cutting off from the field of sentience the stomach, lungs, and heart. The diaphragm, however, appears in all these instances to have retained connection with the brain.[1] Control over the possible compromising of the situation by earlier experience laid down in the cortex was attempted by experiments on nine-weeks-old puppies, whose behavior it was thought would not be due to previously acquired cortical habits. For the considerations which I wish to urge this point is not very important one way or the other. But I doubt whether our animal psychologists would wish to lay any great stress on the emotional expressions of the average nine-weeks-old puppy as being free from the influence of previous experience.

The author concludes that "These experimental observations give no support to the theories of the production of

[1] The importance of the diaphragm in human emotion is so well recognized as to require no emphasis. C. J. Herrick in his 'Introduction to Neurology' says (p. 259) of Sherrington's observations: "The experiments are not very convincing and the problem is probably too complex for solution by so simple means as those here employed."

emotion quoted at the opening of this communication" (James, Lange, Sergi). "On the contrary, I cannot but think that they go some way toward negativing them. A vasomotor theory of the production of emotion seems at any rate rendered quite untenable." (This criticism applies to Lange and largely to Sergi.) "I would not be thought to impugn the importance of the study of such organic phenomena in connection with emotional mental states. The only respect in which the here given observations affect the position of affairs is that they, I think, render it necessary to attribute to these elements of emotion another significance than that imputed by the authorities quoted in my opening paragraph."

The author returns to the issue in his 'Integrative Action of the Nervous System' (pp. 255–258), after recognizing three possibilities, (1) that the psychic part of emotion arises first, and its neural correlate then excites the viscera, (2) that the same stimulus concurrently excites the mind and the nervous centers controlling the viscera, (3) that the emotional stimulus acts first on the nervous centers controlling the viscera whose reaction, as we apprehend it, is the emotion. James's view is identified with the third proposition, whereas Sherrington expresses himself as favorable to the first possibility in these words: "We are forced back toward the likelihood that the visceral expression of emotion is *secondary* to the cerebral action occurring with the psychical state. . . . We may with James accept visceral and organic sensations and the memories and associations of them as contributory to primitive emotion, but we must regard them as reënforcing rather than initiating the psychosis."

Reduced to its simplest terms I think Sherrington's view may be said to rest on the observed fact that judged by the behavior of the head and fore-limb segment of dogs which have undergone cervical spinal transection, fear, anger, pleasure, and disgust are not discernibly modified. This means that judged by such evidence the motor impulses which under the excitement of these emotional conditions normally pour into the muscles of the head and fore-limb region, are not notably reënforced by impulses from the visceral region, so

that the subtraction of the latter occasions no obvious enfeeblement of the former. How much the animals' consciousness may have been modified under the experimental conditions it is, as the author frankly recognizes, quite impossible to determine.

Moreover, strictly considered, Sherrington's conclusion that the visceral and organic sensations are secondary and not primary in emotion seems to be quite imperfectly supported by the evidence of his experiments. It is commonly alleged that some objects call out instinctive responses such as terror or fear in advance of any intelligent knowledge about them and that emotion is involved in a primitive way in the first experience of this sort. This is James's assumption, which I do not understand Sherrington to call in question. Loud sounds thus terrify some infants, fur is alleged to frighten others. James would hold that this drainage of sensory impulses into reflex motor pathways sets up sensory-affective waves which come to consciousness in a vivid way and combine with the residual perceptive processes cognitive of the object occasioning the disturbance. The time taken for this instinctive motor discharge to be reflected back into the sensorial region of the cortex is, in the case of the striped muscles, the fraction of a second; in the case of the unstriped muscles and glands somewhat longer, but even so, a very brief space of time is involved, so brief that the purely introspective problem of determining the order of events in the emotions of mature persons has generally been abandoned as unfruitful. In some instances the bodily reaction seems to come distinctly after the cognitive apprehension of the emotion-provoking stimulus; sometimes it seems to precede, sometimes to accompany. This on the basis of introspection merely. We have no conclusive evidence as to the extent to which these instinctive pathways originally involve the cerebral cortex. Experiments of the transectional kind indicate that most of them are cortical or at least brain affairs; but that possibly rage and some forms of fear may gain expression without the brain. On the whole it seems probable that in man practically all the emotions, even in their first exercise, implicate cerebral reflex arcs. In this sense the actual physiology of the original

emotional excitement is perhaps this: sensorial excitation, cortical or thalamic stimulation and immediate reflex motor and glandular innervation, occupying the fraction of a second; return wave of organic and kinesthetic sensory elements, occupying from a fraction of a second to several seconds, but ultimately fusing with already existing perceptual activity in the cortex; the entire state constituting the full-fledged emotion.

Evidently after a few emotional seizures, memory might begin to operate to reinstate vestiges of old excitement, even in advance of the arrival of some of the reflex organic sensory waves. James's point in this connection would be that we do not have the full characteristic feeling of emotion until this return wave from the organism has been registered. But evidently in many cases this may occur so quickly that we should not discern its temporal separation from the original stimulus.

The facts which Sherrington cites regarding the unmodified reflexes in the head end of the organism, regardless of visceral extirpation, can hardly be regarded as bearing crucially on this question of the order of events in the reflex circuit.

Turning now a moment to Cannon's position, we find him, on the basis of his interesting experimentation, stating in his volume entitled 'Bodily Changes in Pain, Hunger, Fear, and Rage' (pp. 279–80): "If various strong emotions can thus be expressed in the diffuse activities of a single division of the autonomic—the division which accelerates the heart, inhibits the movements of the stomach and intestines, contracts the blood vessels, erects the hair, liberates sugar, and discharges adrenin—it would appear that the bodily conditions which have been assumed by some psychologists to distinguish emotions from one another must be sought for elsewhere than in the viscera." "We do not 'feel sorry because we cry,' as James contended, but we cry because when we are sorry or overjoyed or violently angry or full of tender affection—when any one of these diverse emotional states is present—there are nervous discharges by the sympathetic channels to various viscera, including the lachrymal glands. . . . For this reason

I am inclined to urge that the visceral changes merely contribute to an emotional complex more or less indefinite, but still pertinent, feelings of disturbance in organs of which we are not usually conscious."

Dr. Cannon's statement of the causal sequence of events here may be correct or incorrect, but his conclusion seems to be an instance of argument by affirmation. Certainly there are nervous discharges into the viscera in emotion. On this Professor James and he agree with all the rest of us. But Dr. Cannon surely advances no evidence which touches the relative correctness of his own as against Professor James's estimate of the relation of this fact to the causation of the total emotional psychosis; and his selected instances of similar visceral activities found with distinguishable emotions can hardly be taken alone to afford the needed confirmation. James himself goes even further and comments upon the similarity of *peripheral motor* reflexes (*e. g.*, weeping) in quite dissimilar emotions such as grief and joy.

"This view that the differential features of emotion are not to be traced to the viscera is in accord with the experimental results of Sherrington who demonstrated that emotional responses occur in dogs in which practically all the main viscera and the great bulk of skeletal muscles have been removed from subjection to, and from influence upon the brain, by severance of the vagus nerves and the spinal cord. In these animals no alteration whatever was noticed in the occurrence, under appropriate circumstances, of the characteristic expressions of voice and features indicating anger, delight, or fear. . . . Evidence from the uniformity of visceral response and evidence from the exclusion of viscera are harmonious, therefore, in minimizing visceral factors as the source of differences in emotional states" (Cannon, ibid. pp. 280–281).

Clearly the main point Dr. Cannon has to make, in addition to those already urged by Sherrington, concerns the question whether emotions, assuming their intrinsic dependence upon some sort of organic excitation, can be differentiated and classified on the basis of their visceral components. To this inquiry he gives a negative reply because in all his experiments

in which various emotions have been elicited from his experimental animals, he finds the viscera implicated in ways which he regards as highly similar even when the emotions provoked are presumably quite divergent.[1]

Professor James, so far as I recall, nowhere set himself the task of attempting to differentiate emotions on exactly the basis suggested by Dr. Cannon's statement. Quite the contrary; he says ('Principles,' Volume II, p. 454): "Now the moment the genesis of an emotion is accounted for, as the arousal by an object of a lot of reflex acts which are forthwith felt, we immediately see why there is no limit to the number of possible different emotions which may exist, and why emotions of different individuals may vary indefinitely both as to their constitution and to the objects which call them forth. For there is nothing sacramental or eternally fixed in reflex action. Any reflex effect is possible, and reflexes actually vary indefinitely, as we know." That there are marked differences even in the visceral processes is perhaps sufficiently suggested by Pavlov's classical experiments on the flow of gastric juice which is provoked by hunger coupled with pleasurable emotion, and checked by fear or rage. This checking by fear and also by rage is cited by Cannon as an instance of an identical visceral reaction occasioned by different emotions (p. 277), because, as he says, "These emotions accompany organic preparations for action and . . . because the conditions which evoke them are

[1] If it should prove possible, on the lines suggested by the work of several contemporary physiologists, to differentiate emotions on the basis of the glandular products thrown into the blood, we might hope for a classification which would be on a more stable basis than any hitherto proposed. But even such procedure would in no wise prejudice the validity and importance of the issues raised by James, and it must be admitted that at present the difficulty urged by Dr. Cannon against accepting visceral activities as a valid basis for differentiating emotions from one another, holds in aggravated form. In other words, if emotions introspectively quite distinct have many glandular activities in common, we must either look to other glandular processes yet undiscovered to furnish the lines of demarcation, or abandon hope of a classification which will relate itself in any organic way to our common differentiations based on introspection and world-old opinion. To group together anger, fear, and jealousy, for instance, because they may perhaps possess common glandular features is not to furnish a classification likely to have much psychological interest. On the other hand, we must not prejudge unfavorably the possibilities of future physiological research in this field. The present writer would be particularly unwilling to seem unappreciative of the illumination afforded the general phenomena of emotion by recent physiological discovery.

likely to result in flight or conflict." James himself, it should be added, regards these two emotions as very similar and closely related.

James would certainly have to hold that, however much emotions may vary in their expression in different individuals and in the same individual, when *all* the reflex effects were taken into account, distinguishable emotions would always have variant bodily expressions. But this does not by any means preclude a considerable matrix of substantially identical visceral excitement for some different emotions. Their distinction from one another in such cases may be found in extra-visceral conditions, and particularly in the tonus of the skeletal muscles. Fear and joy may both cause cardiac palpitation, but in one case we find high tonus of the skeletal muscles, in the other case relaxation and the general sense of weakness. Many other illustrations of the same sort of thing will suggest themselves.

Exactly the same evolutionary reason which may explain very *divergent* peripheral motor phenomena in emotion as being adaptive to variant physical situations, may explain the highly *similar* visceral reactions upon which Dr. Cannon comments. Fear and rage may demand similar or even identical expressions of power-generating actions in the viscera, while justifying very different motor reactions at the periphery, and in point of fact this is just what we find. The evolutionary explanation is notoriously inadequate in the light of present knowledge, or better in the darkness of present ignorance, to account for *all* the phenomena of behavior. Yet no one wishes to throw it into the discard, and if it is played as Cannon plays it for visceral activities, there is no logical ground for refusing to use it in the case of the periphery.

We would therefore submit that, so far as concerns the critical suggestions of these two physiologists, James's essential contentions are not materially affected. That the instinctive impulses in the head segment of the dog should be literally unaffected by the removal of the central connections with the viscera is striking evidence of the physiological independence of this anterior segment; but it certainly affords no evidence that visceral organic sensations of human beings play no part

in emotional psychoses—a conclusion which Sherrington does not draw in this extreme form, but which careless readers have so drawn and which is flagrantly at variance with daily experience.[1] Nor does it prove in any way that the psychic state dubbed "emotion" precedes its "physiological expressions" so-called, a view for which Sherrington declares his preference. It is consonant with such a view, but also with James's view. Moreover, so far as the animals may be supposed to have been conscious of the reaction of the facial and fore-limb muscles, they had a good bit of the basis of the psychic stuff which James is always presenting, in season and out, as among the most essential features in our awareness of the self. In other words, no evidence which left facial and cranial muscles unimpaired would ever have seemed to him very convincing as ground for conclusions unfavorable to his theory. And as to the classification of emotion, he was a latitudinarian, holding that many groupings were possible and significant. To group on the basis of visceral reactions alone would presumably have seemed to him theoretically possible, but quite unimportant for any penetrating apprehension of the total situation, because disregarding a full half, and perhaps more, of the important organic reactions, to wit, the general peripheral and skeletal conditions.

James himself, could he but participate in this discussion, would, I am sure, be immensely more interested to discover and define the real facts than to justify any mere theory of his own. And I think he would perhaps urge that, after all controverted points were left aside, the main issues for which he would wish to contend are (1) the instinctive basis of emotional reactions, and (2) the invariable re-percussion upon the cortex of these reflex effects in the muscles, glands, and viscera. Phrase your doctrine so that these two great groups of facts are recognized and properly evaluated, and you may call your theory Jamesian or not as you please. You will at least have accepted what lies at the root of James's theory.

[1] The retention of the expressions of rage in decerebrate dogs suggests the extent to which instinctive motor phenomena may be preserved in the presence of operative injury which might well be supposed to disintegrate all emotional phenomena, and especially in view of the fact that in such animals other emotional expressions are practically abolished.

HEAD'S THEORY OF CUTANEOUS SENSITIVITY

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On the basis of an experiment in nerve division, Head *et al.* have formulated a theory of cutaneous sensitivity which antagonizes the older conceptions based upon the work of von Frey, Goldscheider, *et al.* Owing to the wide and enthusiastic acceptance of the theory in many quarters, it is believed that a critical review[1] may be of service to students of psychology.

Head's first observations concerned the recovery of cutaneous sensitivity in patients in whom cutaneous nerves had been injured or severed. To obtain more reliable results, Head sectioned a branch of a cutaneous nerve innervating a part of his hand and forearm, and thoroughly examined the sensitivity of this part before and after the operation and during recovery. On a certain region of the area affected he found a total insensitivity when the skin was lifted away from the deeper lying tissues, but this area did mediate certain modes of sensitivity when in the normal position. He concludes that this sensitivity belongs to the deeper tissues and not to the skin itself. This is termed deep sensitivity, and it comprises an appreciation of a contact stimulus giving skin deformation, pain due to extreme pressure, muscular activity, and the roughness and locality of contact stimuli. Granted the facts, the conclusion seems valid that this sensitivity is mediated by afferent fibers other than those innervating the skin. This aspect of Head's work contributes nothing essentially new, though the evidence is much more decisive and clear cut than that given by previous methods.

This paper is concerned with cutaneous sensitivity proper.

[1] The summary given in this review of the experimental results and the theory as formulated is based upon the account as given in two of the Head papers. These are entitled "A Human Experiment in Nerve Division" by Rivers and Head, and "The Afferent Nervous System from a New Aspect" by Head, Rivers, and Sherren. Both papers were published in *Brain* (1905, 1908).

Head's theory assumes that peripheral nerves contain two kinds of afferent fibers innervating the skin. These are termed protopathic and epicritic. Roughly these two kinds of fibers differentiate themselves in the following respects:

1. Each has a certain specificity of function. The protopathic mediate pain, hair sensitivity, cold to stimuli of 26° C. and below, and warmth to stimuli of 37° C. and above. These possess a punctuate distribution on the skin, react with a subjective intensity greater than the normal, are rather disagreeable and poorly localized, and are prone to stimulate reflex activity. The epicritic mediate contact to stimuli not producing skin deformation, and cold and warmth to the intermediate ranges of temperature stimuli. The contact is distributed in spots, but the cold and warmth are not. These responses are accurately localized and mediate two-point discrimination.

2. They differ in phylogenetic age. The protopathic system is the older of the two.

3. They differ in extent of distribution. The protopathic has the wider distribution. Both are present in most parts of the skin, but the protopathic alone is found in the viscera and some parts of the skin.

4. They differ in their mode of distribution. The protopathic fibers belonging to different spinal nerves overlap to some extent in their cutaneous distribution, while this is not true of the epicritic fibers.

5. They differ in their regenerative capacity. The protopathic recover more quickly, and their time of recovery is a constant (about 7 weeks) irrespective of the length of the regenerating part. The recovery time of the epicritic fibers varies directly with the distance of the section from the peripheral end of the fiber.

6. Head further maintains that this dual division of conducting mechanism exists only for the afferent nerves—neurones of the first order. After the first synapse in the cord, there are four anatomically distinct conducting mechanisms, each possessing a specific conductive capacity. The functional differentiation here corresponds to the four qualities of contact, pain, cold, and warmth.

It has long been known that the four qualities of pain, contact, cold, and warmth are independent variables in spinal cord diseases as well as in such functional disturbances as hysteria; moreover, this independence has usually been explained in terms of an anatomical differentiation of conditioning mechanisms. Head's hypothesis of four spinal cord tracts is not based upon facts derived from his experiment in nerve division but upon these well-known dissociations due to spinal cord lesions. This aspect of the Head theory thus offers nothing new in the way of fact or theory, with the exception of his suggested localization of the tracts.

The Head conception antagonizes the older view only as to the peripheral mechanisms involved in cutaneous sensitivity. Previous studies on the normal skin had found that cold, warmth, contact, and pain were independent variables as to their distribution, and this fact is usually conceived on the assumption that there are four specific mechanisms which vary independently of each other in their distribution. Two conceptions are now possible: the differentiation may obtain for the end-organs alone, or for both end-organs and fibers. The first hypothesis assumes the existence of four specific kinds of receptor, but all fibers are alike in kind, conducting any impulses given them. According to the second conception, both fiber and receptor are specific in function, and there are four kinds of fiber in the nerve as well as four kinds of end-organ in the skin.

Head's formulation contradicts both possibilities of the older theory as to number of end-organs and fibers. He assumes two specific fibers rather than four or one. As to number of end-organs in the skin, Head makes no definite statements, but seven are demanded by the implications of the theory. The protopathic fibers mediate four functions, and the epicritic three. These functions common to a fiber differ not only qualitatively but also as to their distribution in the skin, and logical consistency demands a corresponding anatomical differentiation. Either each kind of fiber can be further subdivided, or the distinctions in function are conditioned by receptor differences.

The factual data upon which the theory is based consist of certain dissociations of the cutaneous functions. Head rightly admits that in order to prove his hypothesis of a dual nerve mechanism these data must fulfill two conditions. (1) The various qualities must fall into two independently variable groups: either group may be present while the other is absent. (2) Within the group all functions must invariably occur together: no internal dissociation is possible.

The first criticism is one of fact—the contention that the data do not meet the logical requirements of the theory. Head describes the sensitivity of six areas in support of his doctrine, and these results will be canvassed in order. Hereafter we shall use the prefix Ex. in referring to the two temperature qualities belonging to the protopathic system, as these are responses to extreme temperature stimuli. The prefix Md. will likewise designate the epicritic temperature functions as they respond to the medium range of stimuli.

1. *Marginal Zone.*—There was found immediately after the operation a small zone of peculiar sensitivity lying between the central insensitive area and the surrounding regions of normal sensitivity. The sensitivity of this marginal or intermediate zone was termed protopathic; hence the existence of pain, Ex. warmth, Ex. cold, and hair sensitivity is to be expected.

This expectation is hardly justified by the data as given. The operation produced an analgesia over a certain region. Outside of this region the sensitivity for pain was normal. For the most part the boundary between normal pain sensitivity and the analgesia was sharp and clear-cut and could be represented by a line. In other places the transition was gradual and not abrupt. The same situation obtained for the functions of contact, cold, and warmth. Generally speaking the gradual transitions were found on the forearm for all functions, while the hand exhibited boundaries of the linear character. It was thus possible on the hand to define the relative extent of the anesthesia for the four functions, and it was found that usually the four boundaries did not coincide. The relation between pain and contact is thoroughly described

in the text and well represented in the various figures. Where definition was possible, the boundary for pain was found to lie about 2 mm. within that for contact, except for a small area on the wrist where the reverse situation obtained. In the first case the area between the two boundary lines constitutes the marginal zone; because of its shape the second region is called the triangular area and this will be discussed later. The relation of the temperature boundaries to those for pain and contact along the marginal zone is not depicted in the figures and but few descriptive statements are given in the text. Fifty days after the operation the following record is given: "The loss of sensation to cold corresponded in extent with that of the loss of prick; but wherever the part was feebly sensitive to the latter stimulus, sensibility to cold seemed to be absent. To all degrees of heat the borders of the loss of sensation had remained unchanged, and the extent of the anesthesia even to temperatures between 50° and 60° C. uniformly exceeded that of the loss to prick." Later it is asserted of the region near the base of the thumb that "the border of the thermo-anesthesia probably lay slightly within that for sensibility for cotton wool." From these statements it is evident that the temperature boundaries did not coincide with those for pain or contact but probably lay between them. From this definition of boundaries it follows that this marginal zone lacks touch but mediates pain; that its outer half gives normal temperature responses while the inner half is thermo-anesthetic. This is not protopathic sensitivity as alleged. The descriptions of the sensitivity of this zone assert the absence of contact and the presence of pain and hair sensitivity; no mention is made of cold and warmth. In his discussion of the zone (pp. 46–47), Head speaks of a similar area which appeared three weeks after the operation and he admits that this surface was insensitive to all thermal stimuli. The ascription of protopathic sensitivity to the marginal zone presupposes that the operation produces a dissociation within the temperature functions, that the regions of thermo-anesthesia and normal temperature sensitivity were not separated by a line but by an intermediate strip which responded only to the extremes of

temperature stimuli. Such a strip was present after the thermo-anesthetic area had partly recovered its temperature functions, but this is a recovery phenomenon, and was not due to the operation. I have not been able to find any statement to the effect that such a zone of dissociated temperature sensitivity existed after the operation and before recovery commenced. On the other hand, many statements are made which indicate that the transition from normal sensitivity to thermo-anesthesia was abrupt.

2. *Glans Penis.*—The sensitivity of this area is alleged to be protopathic. Head found merely pain and Ex. cold. Ex. warmth was not present, and Head explains the exception in terms of the small size of the area and the usual paucity of warm spots. Hair sensitivity was absent but this lack forms no valid exception because of the absence of hairs.

3. *Viscera.*—Protopathic sensitivity is asserted of these regions. The description mentions only Ex. cold and Ex. warmth. Pain is not mentioned but the existence of this function is probably assumed, as practically all observers report that pain is mediated by these surfaces. Hair sensitivity is absent but this lack can not be regarded as a valid exception.

As is the case of the glans penis, these results on the viscera were not obtained from the experiment in nerve division. They were obtained from hospital patients. Head admits that the observations of hospital patients can not be wholly trusted; for this reason he operated upon himself in his study of cutaneous sensitivity rather than rely upon the observations of patients with nerve sections. It is thus possible that the extreme temperature stimuli applied to the viscera may have been identified by means of pain. Head admits that he himself in the earlier part of his experiment found some difficulty in avoiding this confusion (p. 28). There has been much experimentation and discussion as to the sensitivity of the viscera and pronounced differences of opinion obtain. It is not the purpose of this paper to evaluate the relative validity of the different findings. We merely wish to call attention to the fact that the sensitivity of the viscera is a moot question.

4. *Triangular Area.*—As previously mentioned the boundary of analgesia after the operation lay outside that of touch anesthesia for a short distance on the wrist. The region between these two boundaries because of its shape was termed the triangular area. Its dimensions were approximately 3 × 4 × 4.5 cm. It is asserted that this area manifested epicritic sensitivity immediately after the operation.

The existence of this area after the operation is an assumption, for it was not discovered until the removal of the surgical dressing 28 days after the operation. Head's assumption may be correct, but likewise the sensitivity of this area may be a recovery phenomenon as recovery was noted in other areas as early as 43 days.

It is asserted in several places that this area was totally insensitive to all forms of pain and cold stimuli; no painful or cold responses were ever obtained. The functions present were contact, hair sensitivity, and warmth to stimuli of 42°–48° C. In the second paper it is stated that warmth was elicited from stimuli of 36°–45° C. This list of functions deviates from the epicritic group in three important respects; it includes hair sensitivity, lacks Md. cold, while the stimulus range for warmth is more like that of the protopathic than the epicritic system, for the protopathic warmth is supposed to respond to temperatures above 37° C.

5. *Horsley Case.*—In the second paper Head refers to a case of dissociation resulting from an operation by Sir Victor Horsley. In the removal of a small tumor from the nerves at the base of the brain there resulted a disturbance of sensitivity over the one half of the face. The sensitivity remaining is termed epicritic. The functions noted as being present were contact and warmth in response to temperatures up to 43° C. This list conforms to the epicritic group with the exception of cold, of which no mention was made. One must note that the character of the lesion in this case is not stated. Possibly the conditions are not at all similar to a nerve section as in the Head experiment.

6. *Stages of Recovery.*—Two stages of recovery are asserted for the insensitive area. The protopathic system began to

appear in seven weeks after the operation while the epicritic group did not develop until the end of a year. One would gain the impression from this formulation that simultaneity of recovery obtains for all functions within each system.

The facts hardly justify this expectation. The order of recovery for the various functions is difficult to determine from the account as the description refers to different parts of the affected area and the time of recovery varies with the locality. I have been able to find the time of the first appearance of each of the seven functions for the same locality, viz., the proximal area of the forearm. These times in days are: pain, 56; hair, 86; Ex. cold, 112; Ex. warmth, 161; contact, 365; Md. warmth, 407; and Md. cold, 440(?). The only fact justifying a division into two groups, as was done by Head, is the longer time interval between the first appearance of Ex. warm and contact (204 days) than between any other two functions, viz., 49 days. Moreover, simultaneity of appearance does not obtain for the functions within the alleged systems. Since the functions of a group belong to a common kind of fiber, one would expect them to return practically at the same time. Several logical possibilities are open to Head in order to explain this temporal order of recovery within a system. (1) One can assume that it is due to the varying rates of regeneration of different fibers of the same kind. This assumption for the protopathic fibers would contradict the fifth proposition of his theory. Moreover, it would not account for the definite order of recovery of the seven functions. (2) The order of return within a system may be due to differences in the rate of recovery of the end-organs or receptors. There is good reason for supposing that the functional capacity of these receptors has been affected by the operation. End-organs tend to degenerate when their nerve is severed, and pronounced abnormal conditions of the cutaneous tissues were noted in the experiment. If recovery of end-organ as well as of fiber is necessary, it is logical to suppose that different rates of recovery would obtain for different structures. On this assumption Head's dual division of fibers is unnecessary to explain the facts of recovery. Why assume differences of both fiber and receptor

when either one alone will do as well? In other words, these facts of recovery can be conceived as well on the basis of seven mechanisms as in terms of two.

The assertion of two separate stages of recovery would seem to imply that there exists between the two a period in which no development occurs. As a matter of fact Head makes no assertions as to definite stages of this character. He admits that recovery is both gradual and continuous. Each function at its first appearance is defective and there is gradual improvement until normal conditions are reached. None of the protopathic group of functions attained complete recovery before the beginning of epicritic development. The periods of recovery of the two systems overlap to a large extent. Head merely contends that the protopathic system *begins* its development earlier than does the epicritic. Their final synchronous improvement is admitted.

The pertinent facts concerning order of recovery are these: The seven functions appear in a definite temporal order, one interval (204 days) between two successively appearing functions is much greater than the others, the recovery of each function is a gradual process, and all progress together in the later periods of recovery. These facts would indicate that a dual division is somewhat arbitrary. If stages are demanded, the assumption of seven is as feasible as Head's dual division.

The facts adduced from the above six lines of evidence are tabulated in such manner (Table I.) that one can easily observe to what extent these data fulfill the two conditions previously enumerated. The first column groups the seven functions into the two alleged systems. Columns 1–5 refer to the first five areas discussed. The mark X opposite any function signifies the presence of this function in that area. A dash (—) means the absence of the sensation. A question mark (?) indicates some doubt as to the fact. In two cases the space is left blank; these refer to the hair sensitivity of the viscera and the glans penis. Since the absence of hair sensitivity from these two regions forms no valid exception to the theory, the reader may regard these as though they were crossed. The sixth column states the time in days of the

first appearance of the seven functions for the proximal area of the forearm.

An inspection of this table reveals the fact that the data do not meet the logical requirements of the theory. There is one exception to the independent variability of the two groups.

Table I

Dissociated Functions

Functions	Marginal Zone	Glans Penis	Viscera	Triangular Area	Horsley Case	Stages of Recovery	Dentine and Tooth Pulp	Kiesow's Area	Cornea	Conjunctiva
	1	2	3	4	5	6	7	8	9	10
Protopathic:										
Pain	X	X	X	—	—	56	X	—	X	X
Hair	X			X	—	86				
Cold Ex.	—	X	X	—	—	112	—	X	X	X
Warm Ex.	—	—	X	—	—	161	—	X	—	X
Epicritic:										
Contact	—	—	—	X	X	365	—	X	—	—
Warm Md.	—	—	—	X?	X	407	—	X	—	X
Cold Md.	—	—	—	—	—	440?	—	X	X	X

The main deviations concern the dissociations within the groups. There are three cases of dissociation within the protopathic group and two for the epicritic. In the alleged protopathic areas essential functions are absent in three cases. In the two epicritic areas cold is absent both times. The importance of these dissociations within the groups must be emphasized. Head rightly maintains that a dissociation into two independently variable groups must be interpreted in terms of two distinct anatomical mechanisms. Likewise logic would demand that a similar dissociation within the alleged systems means additional mechanisms. In considering the data, the reader must dismiss all bias in favor of any definite number of mechanisms. One has as much *a priori* right to assume seven or four kinds of fiber as two. Our conclusions must be based wholly upon the evidence as given in the table. The question at issue concerns the number of independent variables. The writer is much inclined to believe that if a number of persons without knowledge of Head's theory were confronted with the given facts and asked to

determine the number of independent variables, very few of these would make such a division as that of Head. I am inclined to believe that the majority of such persons would conclude in favor of six or seven variables. In other words, the facts as given by Head do not support his theory; rather they can more easily be explained in terms of seven mechanisms, even though we accept the facts at their face value. The reader must remember that the validity of some of the facts may be questioned.

A second objection concerns the doctrine of a constant time for protopathic recovery. Head assumes that the time of recovery of the protopathic system is a constant irrespective of the location of the nerve section. He asserts that functional recovery depends upon fiber regeneration and that the time of regeneration of the protopathic fibers is a constant irrespective of the length of the regenerating part. This assumption is somewhat illogical if regeneration means actual growth through space. The doctrine is also controverted by the facts concerning the spatial order of recovery over the insensitive area. From the figures and the descriptions representing the progression of events it is evident that recovery first occurs for points nearest the nerve section and gradually extends toward the distal portions. This order obtains for both the protopathic and the epicritic systems. Since the regenerative length of those fibers ending in the distal portions is greater than for those innervating the proximal parts, this spatial progression is to be expected if the time of regeneration is a function of distance. According to Head's assumption, protopathic recovery should be uniform throughout the affected area. The facts indicate that the time for regeneration for all fibers is a function of distance, and one of the main arguments in favor of two kinds of fiber is thus invalidated.

Another spatial order of recovery is apparent from the account. Recovery progresses from the edges of the normal zone in toward the center of the insensitive area. The size of the affected area is gradually decreased by a contraction of its boundaries. This fact can not be explained by any of the assumptions of the theory. The phenomenon can be

explained by the hypothesis that the order of recovery is a function of the receptors. As we have noted, there is good reason for supposing that nerve section will produce a functional incapacity of the end-organs. Naturally the functional recovery of the receptors will occur after the regeneration of the fibers. Since this incapacity is a result of the abnormal trophic conditions of the cutaneous tissues, the degree of the disturbance will increase from the edges of the normal zone in toward the center of the affected area, for the nutritive condition of the marginal areas will depend in part upon the trophic condition of the adjoining normal zone. The receptors of the marginal areas, being less affected, will recover first after the recovery of the fibers.

A final objection concerns the application of the theory to peculiarities of functional distribution in the normal skin. Both kinds of fiber are found in most regions of the skin, but occasionally either may be absent. The sensitivity of peculiar areas is thus explained. All anomalous areas must possess either epicritic or protopathic sensitivity. The peculiar sensitivity of the viscera and the glans penis is thus due to the absence of the epicritic system. Granted the validity of the theory as applied to these two regions, difficulties are encountered in an extension to other areas.

1. The dentine and tooth pulp are said to mediate only pain. This fact can not be subsumed under Head's conception.

2. Pain is absent from Kiesow's cheek area. The sensitivity of this area could be regarded as epicritic if it were demonstrated that temperature spots were absent and that the area reacted only to the median range of temperature stimuli (26°–37° C.). It is doubtful if this restricted range of sensitivity obtains.

3. In the cornea only cold and pain are said to exist. This deviates from protopathic sensitivity in two respects. All sensitivity to warmth is lacking, while the range of sensitivity for cold is not restricted to stimuli below 26° C.

4. In the upper conjunctiva contact alone is said to be lacking. This form of sensitivity could be regarded as protopathic if the temperature spots did not react to stimuli of 26°–37° C.

The sensitivity of these four areas is listed in columns 7 to 10 of Table I. The evidence from these regions as well as that from the viscera and the glans penis must be considered in determining the probable number of independently variable systems. Considering the distribution of the seven functions in all of the areas the evidence in favor of two systems is far from conclusive.

In conclusion, the facts of dissociation speak in favor of seven sensory mechanisms, and such a conception will easily explain the spatial and qualitative orders of recovery. On this hypothesis one can assume a differentiation of both fiber and receptor, or limit it to the end-organs exclusively.

So far we have followed Head in his assumption that seven sensory functions were obtained by his experiment. We may now raise the question of the possibility of reducing these seven functions to the conventional four, viz., contact, warmth, cold, and pain.

1. It is possible that the hair and contact sensitivity represent but two stages in the recovery of the same mechanism. Both senses possess a punctuate distribution, and the two kinds of spots are for the most part identical in location, *i. e.*, over the hairs. Also, no case was given in which contact was present while hair sensitivity was absent. Hair sensitivity may thus be the defective response of the contact mechanism while in an abnormal condition.

2. The same conception will easily apply to the two cold sensations. Protopathic cold may represent but the first stage in the recovery of the cold mechanism. As a consequence of its defective condition, this mechanism will at first possess a high intensity limen; the spots will respond only to extreme cold stimuli and when these are placed directly over the receptor. As recovery progresses, the limen is gradually lowered and responses will occur for the medium ranges of temperature stimuli and for the areas between the spots. The only crucial exception to this hypothesis would be the presence of the epicritic cold in an area in which the protopathic cold is absent. No such case was found, for in both the alleged areas (the triangular area and the Horsley case)

no cold responses of any sort were obtained. Some positive evidence in favor of the conception of a single mechanism may be adduced; when protopathic cold first appears its intensity limen is much higher than 26° C., and this limen is gradually lowered until 26° is reached. Likewise, the epicritic cold starts with a limen near 26° and this gradually sinks to the body temperature. In other words, we are probably dealing with a single mechanism whose intensity limen is gradually decreasing with recovery, and the point 26° C. represents but an arbitrary boundary between two stages of recovery. In support of this conception the following quotations[1] are apropos: "The terminal phalanx of the thumb was certainly sensitive to cold below 17° C." (p. 12). "At first, during the earlier protopathic stages, no sensation of cold might be evoked by temperatures above 20° C.; but with rapid increase of this form of sensibility, the part became increasingly sensitive to temperatures up to 24° C. With this gradual improvement, the first-grade spots responded with increasing constancy to the same range of temperature. Five years after the operation, an area between the knuckles of the index and middle fingers, though highly sensitive, still remains in a purely protopathic condition. Within its limits lie many active cold-spots, but not one of these reacts to temperatures above 26° C., and many scarcely react to 24° C." (p. 60). The following statements are made in describing the return of the epicritic system: "Temperatures of 26.5° C. and 25.5° C. were said to be cool in the neighborhood of the wrist, although no other part of the affected area reacted at that time to anything above 24° C." (p. 73). "When the weather was favorable, temperatures of 27° C. began to be appreciated as cool" (p. 74).

3. The same conception will apply to the mechanism for warmth, though the evidence is more ambiguous. The gradual decrease of the intensity limen is evident from the following statements: "The terminal phalanx of the thumb undoubtedly responded to temperatures above 45° C." (p. 12). "About this time (October 8), the upper patch on the forearm became undoubtedly sensitive to temperatures of and above 45° C."

[1] These quotations are taken from "A Human Experiment in Nerve Division."

(p. 14). "On November 1, the back of the hand reacted for the first time to temperatures above 45° C." (p. 52). "At first, few heat-spots responded to temperatures below 45° C.; but as recovery progressed the first-grade spots began to react to 40° C. Two spots only were sensitive to 37° C. under favorable conditions. Even now that part of the back of the hand which remains in a condition of protopathic sensibility is still insensitive to temperatures below 37° C.; the temperature most favorable for evoking a sensation of uncomplicated heat still lies between about 44° C. and 48° C." (p. 59). Concerning the recovery of epicritic warmth the following statements are made: "On June 5, 1904, the affected area on the forearm responded to temperatures of 37° C. This sensibility to warmth rapidly increased, and on June 26 was obtained, even with 34° C." (p. 16). "Before the skin in the neighborhood of the wrist had become sensitive to cotton wool, we noticed that temperatures of 36° C. or even 34° C. occasionally caused a sensation of warmth" (p. 72). "The whole of the affected area on the forearm has become uniformly sensitive to temperatures above 35° C.; even 33° C., under favorable conditions, produced a sensation of warmth" (p. 75).

This gradual change in the threshold during recovery also obtains for pain. The limen at first was very high; gradual improvement occurred during the protopathic period, but the normal threshold was not attained until long after the beginning of the epicritic stage of recovery.

Two crucial exceptions to this hypothesis of a single warmth mechanism are urged. It is asserted that epicritic warmth was found in two areas in which protopathic warmth was lacking. In the Horsley case the patient stated that a temperature of 55° C. was neither hot nor cold, but that 43° C. was undoubtedly warm. This evidence is hardly decisive as the lesion involved may be of a different order from that of a nerve section, as in the case of Head. The inability to appreciate properly a temperature of 55° C. may be due in part to the absence of pain from this area. Also the range of temperature sensitivity is not strictly epicritic in character. The triangular area was sensitive to stimuli from 42° C. to 49° C.

after the operation. Again, this is not a true epicritic warmth, and the inability to respond properly to the higher temperatures may have been due to the analgesia of this region. These cases present as much difficulty to the Head theory as to the hypothesis of a single warmth mechanism.

In the formulation of a theory of cutaneous sensitivity too much weight should not be given to the facts obtained by the methods of this experiment. As yet little is known of the relation between functional recovery and the regenerative process. The nerve section produced an extremely abnormal condition of the cutaneous tissues. The experimenters are dealing with the functional capacity of mechanisms in a highly abnormal state, with a situation containing a multitude of unusual and *unknown* conditions. Naturally, various peculiarities of sensitivity are to be expected with such a situation, and it is possible that all such peculiarities are to be explained in terms of these unusual and unknown conditions resulting from the operation.

The Head experiment has been attacked on a factual basis. The work has been repeated by several experimenters, and we shall briefly summarize the results of one of these investigations, viz., that of Trotter and Davies.[1] Apparently their work was performed with as much care and attention to detail as that of Head. They sectioned seven nerves in succession in the same individual. Comparison with Head's results is often difficult because of slight differences in method and technique. Many warm responses were rejected as being hallucinatory in character. Contact stimuli were used which were sufficient in strength to awaken deep sensitivity, and introspection was relied upon to differentiate between contact and pressure.

These investigators discovered but four sensory functions. The transition from the region of normal sensitivity to the insensitive area was never abrupt; between the two was found a marginal zone of defective sensitivity. The extent of the anesthesia for the four functions coincided in the main, but their boundary lines were independent variables. A definite temporal order of recovery for the four qualities was not

[1] *Jour. of Physiol.*, 38.

evident; in the main their recovery was synchronous. Touch came first, pain and cold followed quickly, while warmth came in somewhat later. But one spatial order of recovery was noted; recovery first occurred for the points nearest the section, the line of progress followed the nerve trunk and then spread out laterally. Two adjacent branches were once severed, leaving in between the affected areas three small islands of sensitivity which were supplied by a branch not sectioned. In one of these islands was found a small V-shaped area which mediated contact but no pain nor temperature. This area certainly could not be subsumed under either one of Head's two systems of cutaneous sensitivity. These authors conclude that their facts indicate that the skin possesses four independently variable functions and hence they assert the existence of four peripheral mechanisms. It is assumed that the differentiation includes both fiber and end-organ.

It is not our purpose to evaluate the relative validity of the two experiments nor to force upon the reader any definite conclusions. Rather we wish to combat the rather prevalent tendency to a ready and uncritical acceptance of the Head formulation.

THE THEORETICAL FOUNDATIONS OF WUNDT'S FOLK-PSYCHOLOGY

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Wundt's folk-psychology constitutes an integral part of his philosophical system. In some respects it must be regarded as the crowning achievement of his thought, inasmuch as its theoretical foundations presuppose all of his former work. In order to gain a proper setting for our critical considerations, we must begin with a somewhat detailed discussion of certain general concepts employed in Wundt's philosophy. Without a clear comprehension of these a criticism of his folk-psychology would not be feasible.

I. Wundt's Concept of Creative Synthesis

The term 'voluntaristic' characterizes Wundt's psychology—in contradistinction to that of other schools, such as the Associationist, Herbartian, and others—as viewing all psychic phenomena as the expressions of the will, but of the will in the broadest possible sense of the term. To Wundt the will is not a metaphysical concept, as it is to Schopenhauer. It is rather a principle that states the fundamental nature of all psychic life, from the simplest to the most complex processes. It gives expression, in the form of a principle, to the empirical facts about the mind—the psychological facts as they are borne out by an analysis of psychic experience and by an interpretation of psychological experiments.

Wundt regards the sensations (*Empfindungen*) and the feelings (*Gefühle*) as the two ultimate and irreducible elements of psychic life. They do not occur as concrete realities, being the abstract products of logical analysis. In the world of reality we only experience complex psychic phenomena compounded of those elements. The psychic elements are only real in the sense that they are constant components of all

psychic experience. But no psychic phenomenon is merely the sum of the component sensational and emotional elements. Invariably it is something new over and above the sum of the parts that enter into the compound. Thus all temporal and spacial ideas, for instance, are not equal to the sums of the separate sensations and feelings that constitute their elements. A chord is not equal to the sum of all its component tones: it is rather, on its psychological side, a new and unique experience. All psychic phenomena are therefore creative products of synthesis: they are, when seen from this point of view, acts of *will*. This justifies the term 'voluntaristic' as referring to that fundamental trait of the mind by which all psychic processes are creative. As we see, Wundt extends the concept of creative synthesis from the highest forms of apperceptive processes—which we witness in intellectual and artistic pursuits, where the creativeness is so obvious—to the entire realm of psychic life.

The principle of creative synthesis, as the one cardinal principle of psychic life, embraces the three principles of psychic causality which Wundt cites; namely, the principle of psychic 'resultants,' the principle of psychic relations, and the principle of psychical contrasts.[1] They pertain to the same content viewed from different angles.[2]

A word must here be said about the counterpart of the principle of creative synthesis; namely, that of the heterogeny of ends (*Heterogonie der Zwecke*). According to Wundt, every causal relation when reversed becomes a teleological one, inasmuch as the two principles of cognition, causality and teleology, do not exclude each other, but are complementary modes of bringing phenomena into logical relation. Thus the principle of creative synthesis, as a causal principle, can be reversed into a teleological one; namely, that of the heterogeny of ends. It applies to the same facts of psychic life as the former, but from a different point of view. What is cause and effect in one case is means and end in the other. "Es [handelt] sich eben bei dem Prinzip der Heterogonie um kein

[1] 'Outlines of Psychology,' 2d Engl. ed., 364 et seq.; 'Physiologische Psychologie' (5th ed.), 3, 778 et seq.

[2] 'Physiol. Psychol.,' 3, 787.

neues Prinzip, sondern nur um eine durch die besonderen Bedingungen nahegelegte teleologische Umformung der causalen Prinzipien des psychischen Geschehens. . . ."[1] It is important to emphasize this relation of the two principles, in order to understand the significance which Wundt attributes to the heterogeny of ends in the psychic development of the individuals as well as of the group. It must also be stated in this connection that as an interpretative principle of cultural phenomena not the causal principle but rather the teleological one (the heterogeny of ends) is of paramount importance. This is the case because the psychic phenomena in cultural development, on account of their complexity on the one hand and the discrepancy of their causes and effects on the other, are not interpretable progressively from the causes to the effects, but rather regressively from the given ends backwards to the motives. Theoretically the heterogeny of ends is a principle of universal validity. Wundt elaborates its application especially in connection with the interpretation of the development of ethical ideas out of non-ethical or rather pre-ethical ones.

In keeping with the nature of psychic causality, according to which an effect is not equal to its cause, the heterogeny of ends implies that in psychic development the ends attained are not equivalent to the purposes embodied in the motives. Between the motives and their ends there arise as unintended by-products secondary 'resultants' not implied in the motives, and thus is brought about a constant discrepancy of motives and ends. In a chain of motives and ends these unintended resultants, as well as the purposed end, take the form of new motives. Thus a constant shifting of the purposes takes place, which leads Wundt to speak of a heterogeny of ends.

II. The Concept of Psychic Actuality

The concept of creative synthesis is supplemented by that of psychic actuality. This concept expresses the fundamental characteristic of Wundt's conception of psychic life, and distinguishes his psychology from those other forms that conceive

[1] 'Physiol. Psychol.,' (5th ed.), 3, 789–790.

the psyche as a substance analogous to that of the physical world. According to Wundt the phenomena of psychology are *toto coelo* different from those of the natural sciences in their relation to the investigating subject. It is of the very nature of the natural sciences that their objects be thought to exist in an objective world distinct from the subject. They exist as if there were no subject. This conception of the purely objective world of natural phenomena becomes possible through an abstraction from the observing and correlating subject. This abstraction involves the postulation of a physical substratum to which all natural phenomena must be referred as to the underlying principle. The hypothetical substratum thus postulated takes the form either of matter or of energy. In this way the natural sciences view their phenomena through the medium of an auxiliary concept; namely, that of the substantiality of their objects. Over and against this, psychology does not approach its objects by way of an abstraction from the subject. It views them 'immediately' (*unmittelbar*), since they are given in the consciousness of the subject itself. Those psychologists who postulate a soul-substance ignore the intrinsic nature of the objects of their investigation, and have fallen into the pitfall of an untrue analogy of the psychical with the physical world. In contradistinction to physical substantiality, Wundt characterizes the subject-matter of psychology as the actuality of psychic life. This actuality defines 'the nature of mind as the immediate reality of the processes themselves.'[1]

The distinction between physical substantiality and psychical actuality determines likewise that between physical and psychical causality. The terms of the one form of causality are quantitative, those of the other are qualitative.[2] Physical causality is characterized by the quantitative equivalence of cause and effect—a fact which finds its expression in the principle of the preservation of energy. Psychical causality, as implied in the concept of the creative synthesis, involves qualitative disparity of cause and effect. "Es gibt absolut

[1] 'Outlines,' 357.
[2] Wundt, 'Logik' (3d ed.), 3, 276.

kein solches [*i. e.*, psychologisches] Gebilde, das nicht nach der Bedeutung und dem Wert seines Inhaltes mehr wäre als die blosse Summe seiner Faktoren oder die blosse mechanische Resultante seiner Komponenten."[1] By juxtaposing the constant values of physical energy to the creative synthesis of psychic energy, Wundt gains specific psychological connotations for his concept of evolution.

III. The Bearing of Creative Synthesis on the Concept of Folk-Soul

Wundt identifies the growth of psychic life with the increase of qualitative 'Wertgrössen,' which concept he contrasts with that of the quantitative 'Grössenwerte' of the physical world.[2] This clever play of words contains *in nuce* the essence of Wundt's philosophy. The concept 'Wertgrössen' implies the teleological factor which Wundt introduces into his conception of psychic evolution. The idea of purpose (*Zweck*) is an integral part of Wundt's concept of voluntaristic psychology. The intrinsic nature of man is his psychic life. But, as we have seen, all psychic processes are voluntaristic, are products of a creative synthesis—are acts of will, if you like. These processes are therefore, by their very definition, purposive. Thus the consistent conclusion is that the existence of man finds its purpose in the creation of psychic life;[3] but this purpose of existence is not limited to the life of man. The individual human being is but a link in the chain of psychic evolution. Life in the entirety of its expressions is the self-manifestation and self-evolution of the psychic. Nature as the physical prerequisite of the psychic is the 'Vorstufe des Geistes'; and, inversely, the psychic is the 'vorauszusetzender Zweck des organischen Lebens.'[4]

The principle of creative synthesis defines the nature of this psychogenesis in terms of psychic causality. This principle, which, as we have seen, characterizes all our psychic processes as creative productions over and above their con-

[1] 'Logik,' 3, 274.
[2] *Ibid.*, 276.
[3] Wundt, 'System der Philosophie' (3d ed.), 2, 238–239.
[4] *Ibid.*, 147.

stituent parts, characterizes them, in short, as 'voluntaristic,' applies to all psychic life in general, from its lowest to its highest forms. From this point of view Wundt's position in the discussion of the relation of reflexes to volitions obtains its deeper significance. Volitions are not differentiated mechanical reflexes, but rather reflexes are mechanized volitions. Thus in the successive stages of evolution the volitions lay claim to priority. Wundt demonstrates this by the fact that, even in the very lowest forms of life, reactions are not mechanical but purposive, and thus characterized in the same way as our own psyche.

Wundt's theory of psychogenesis gains its immediate significance for the concept of folk-psychology through the fact that the universality of creative synthesis obtains not only for the continuous succession of steps which lead from our psyche down to inorganic nature, but also for that whole sphere of psychic life which, as Wundt assumes, leads beyond the individual psyche into the realm of the over-individual life of the community (der Gemeinschaft). The creative synthesis which characterizes the intrinsic nature of all psychic compounds, and of all interconnections of these compounds in the psyche of the individual, is found repeated, according to Wundt, in a strictly analogous way, but on a higher level of evolution in the psychic life of the community or the folk. The reality of the folk-soul is involved in the extension of this principle beyond the individual psyche. As the psyche of the individual is built up in the form of a progression of superimposed syntheses, so the folk-soul is a synthesis of syntheses: it is something creatively new, not equal to the sum of its elements, that is to say of the individuals of which it is composed.[1] Wundt expresses this idea clearly in the following sentence: "Aber wie nicht psychische Elemente in isoliertem Zustande, sondern ihre Verbindungen und die aus diesen entspringenden Produkte das bilden, was wir eine Einzelseele nennen, so besteht die Volksseele im empirischen Sinne nicht aus einer blossen Summe individueller Bewusstseinseinheiten, deren

[1] 'Darum ist das gemeinsame Leben niemals eine blosse Addition individueller Wirkungen,' *Deutsche Rundschau*, 1891, 200.

Kreise sich mit einem Teil ihres Umfangs decken; sondern auch bei ihr resultieren aus dieser Verbindung eigentümliche psychische und psychophysische Vorgänge, die in dem Einzelbewusstsein allein entweder gar nicht oder mindestens nicht in der Ausbildung entstehen könnten, in der sie sich in Folge der Wechselwirkung der Einzelnen entwickeln."[1]

IV. The Folk-Soul

Wundt maintains that the folk-soul is no less real than the soul of the individual. In order to understand the line of thought that leads up to this assertion, it is necessary to recall what has been said about his conception of psychic actuality. The concept 'soul,' as used by Wundt, does not refer to a substance, be it materialistic or spiritualistic, but rather to the immediateness, the actuality, of psychic experience. Thus the soul of the individual, being deprived of the connotation of a substantialistic substratum, is an abstract term for an entity of concrete psychic experiences. "Unter der individuellen Seele verstehen wir die unmittelbare Einheit der Zustände eines Einzelbewusstseins."[2] This same conception of 'soul' as psychic actuality leads Wundt to postulate the reality of the folk-soul. As we have seen, he defines the folk-psychological phenomena as a sphere of psychical facts, which, while claiming the individual souls as their constituent elements, represent a new and peculiar creative synthesis distinct from the component parts. The concept 'folk-soul' refers in exactly the same way to the entity of these over-individual psychic facts as the individual soul refers to that of the psychic experiences of the individual. The essential connotation of the folk-soul, like that of the individual soul, is that it is an actuality, not a substance. Now the psychic facts of the over-individual group, as empirical facts, are according to Wundt as real as the psychic life of the individual. Therefore, so argues Wundt, the term 'soul' is equally justifiable and equally applicable in the case of folk-psychological phenomena as it is

[1] 'Völkerpsychologie' (1st ed.), I, 1, 9–10; see also 'Probleme der Völkerpsychologie,' 1911, p. 13.

[2] 'System d. Phil.,' 2, 148.

in that of individual psychology.[1] The axiom of voluntaristic psychology is, 'So viel Aktualität so viel Realität."[2] Therefore the folk-soul, as an actuality, is a reality.

A psychology, says Wundt, that abides by the conception of a soul-substance, can never comprehend the reality of the folk-soul, because a soul-substance is necessarily bound to the physical entity of the individual. "Ist die Seele ein beharrendes Wesen, wie die Substanzhypothese annimmt, ein geistiges Atom . . . , so hat selbstverständlich nur das Individuum wahre Realität."[3] To those who state that the folk-soul is a fiction and a production of the mythological imagination, Wundt replies that the conception of the soul as a substance is mythological rather than that of the actuality of the folk-soul, and hence of its reality.[4] The idea of a soul-substance, says Wundt, is a survival of mythical animism.

This is the line of thought that induces Wundt to postulate folk-psychology as an independent science, with its own particular realm of problems. Its existence is as justified as that of individual psychology. Wundt defines it as the study of the folk-soul ('die Lehre von der Volksseele').

Wundt's plea for folk-psychology is apparently founded on a rigid construction of logical thought. The well-balanced succession of premises and conclusions offers a good example of Wundt's argumentative brilliancy. The line of thought is enticing; and still the one decisive point in his argument for the reality of the folk-soul is gained by a subtle *coup d'état.*

The *raison d'être* of folk-psychology is at the mercy of the thesis of the reality of the folk-soul. This reality, as we have seen, is based on the idea of psychical actuality—a concept taken from the psychology of the individual. In individual psychology the concept 'actuality' acquires its meaning through the fact that the phenomena at hand are *immediately*[5] perceived in contradistinction to the mediate cognition of

[1] 'Probl. d. Völkerpsychol.,' 1911, 13, 20.

[2] 'Logik,' 3, 293–294.

[3] 'Logik,' 3, 293; 'Syst. d. Phil.,' 2, 188.

[4] 'Völkerpsychol.,' 1, 1, 8–9.

[5] I use the terms 'mediate' and 'immediate' for Wundt's terms 'mittelbar' and 'unmittelbar.' 'Indirect' and 'direct' would only convey the meaning approximately.

the natural sciences, which must postulate a substance as the extra-subjective substratum of their phenomena. The immediateness of experience is the fundamental connotation of psychic actuality. The condition in question is fulfilled in the case of individual psychology by the intrinsic and irreducible nature of consciousness. But what about the folk-soul? The folk-soul is by definition an over-individual synthesis. The psychic phenomena of the folk-soul are by definition not contained in the psyche of the individuals as such; but immediate psychic experience is—again by definition—confined to the consciousness of the individual. How then can there be an *immediate* experience of an over-individual synthesis? And what sort of a meaningless thing is an over-individual actuality of psychic life? But if there is no over-individual actuality, then there can be no folk-soul. The one falls with the other. The *contradictio in adjecto* which we here encounter in Wundt's argument lies in the following premises: psychic actuality is the immediateness of experience; the folk-soul is an over-individual synthesis. Wundt bridges the gap by ignoring at the decisive point in his argument the 'immediateness' of psychical actuality.

V. The Relation of the Individual to the Group

Having discussed in abstract terms the flaw in Wundt's plea for folk-psychology by pointing out his self-contradictory usage of concepts, let us investigate for a moment somewhat more concretely the same break in argumentation from another point of view; namely, from that of the relation of the individual to the group.

Wundt states categorically that folk-psychology deals with the psychology of language, religion (*Mythus und Religion*), and custom. These three types of cultural phenomena are the achievements *par excellence* of the folk-mind (*Volksgeist*). Not the individuals, but the group (*die Gemeinschaft*), is the creator of language, religion, custom. Of course, the group consists physically of a number of individuals; but those folk-psychological phenomena, so argues Wundt, represent a higher syn-

thesis that transcends the scope of individual consciousness.[1] He says, "Sie [*i. e.*, die Erlebnisse und Erzeugnisse geistiger Gemeinschaften] unterscheiden sich dadurch von den Synthesen des individuellen Bewusstseins, dass sie sich aus Bestandteilen *eines* Bewusstseins niemals erklären lassen, sondern auf einer geistigen Wechselwirkung vieler beruhen, die sich zu den genannten Vorgängen ähnlich verhalten, wie die Vorstellungs- und Willenselemente des Einzelbewusstseins zu den zusammengesetzten Vorstellungen und Willenshandlungen des einzelnen."[2] Misled by the analogy between the synthesis of psychical elements in the individual and the synthesis of the individuals in the group, Wundt becomes entangled in the illusory problem of the relation of the individual to the group.

This problem is no less an illusion than the old one of the relation of the particular to the universal, of which indeed it is but a specific application. The illusory nature lies in the fact that, when speaking of the particular and the universal, we are not operating with concepts of different objects, but rather with different conceptual abstractions of the same object. The individuals are the group, and the group is the individuals. The two terms represent different modes of conceptualizing the same thing. Wundt puts the problem thus: Is the individual as such, or is the group as such, the creator of language, religion, and custom? He states correctly, in opposition to the intellectualistic school of psychology, that it is not the individual as such; but at once he falls into the opposite error, and asserts that the group as such, the over-individual synthesis, is the creator of cultural phenomena. Wundt's position is no more consistent than is that of his adversaries. Logically a collective term is an abstraction from its analytic components, and the latter again are an abstraction from the former. Correspondingly, in our case the group is an abstraction from the individuals, and the individuals are an abstraction from the group. Thus Wundt's juxtaposition of the individuals as such, and the group as the over-individual synthesis, is an absurdity. A result of the contradictions in which Wundt becomes en-

[1] 'Probl. d. Völkerpsychol.,' 24.
[2] 'Logik,' 3, 295.

tangled is the vagueness with which he continually treats the relation of the individual to the group as soon as he attempts to demonstrate this relation concretely.[1]

The error in Wundt's position is determined from the outset by the way in which he formulates his problem: Is the individual as such, or is the group as such, the creator of language, religion, and custom? The difficulty thus involved arises from a confusion of the two distinct points of view from which the individual can be conceived. From the one point of view the individual is the subjective entity as experienced "immediately" (*unmittelbar*) in consciousness. This subjective individual experiences himself as autonomous. It is the individual of psychical actuality and of indeterminism. From the other point of view the individual is society and he is history. He is determined psychically by his cultural milieu. He is the individual of determinism, and the object of culture-history. We avoid the problem of determinism versus indeterminism, and we do not, as Wundt does, entangle ourselves in its meshes, if we distinguish clearly between these two points of view. Wundt gains the concept of the group as an over-individual synthesis by viewing the individual only from the first point of view; namely, as an autonomous monad.[2] Of course the individual as such cannot be brought into *rapport* with culture-historical problems when approached from this standpoint. Since language, religion, and custom are by definition psychical and historical phenomena—in short, cultural phenomena—the only point of view from which their study is conceivable is that of culture-history. But from this point of view, the individual, as we have seen, is history, he is society; he is, in brief, the *ζωὸν πολιτικόν*.

To conceive the individual historically—and by this I mean at the same time socially and culturally—as an autonomous entity is as meaningless as to study the course of a river independent of the geology of its bed. The *ζωὸν πολιτικόν* has *sui generis* a psycho-historical setting, it has a culture. And this culture is nothing accessory, it is not cast in the

[1] See, for instance, Wundt, Menschen- und Tierseele (4th ed.), 509.

[2] 'Syst. d. Phil.,' 2, p. 204.

mould of an autonomous individual; but language, religion, custom *are* from the historical point of view the individual, they *are* the group, they *are* the ζωὸν πολιτικόν.

The result to which Wundt's distinction between the individual soul and the folk-soul leads is nicely borne out by the following analogy, to which he repeatedly calls attention in his different works. In his individual psychology, Wundt designates the ideas and the emotions as the analytic components of the psyche, and defines the third class of psychic phenomena, the volitions, as a synthesis of the former two. Since ideas, emotions, and volitions thus make up the individual soul, Wundt, consistent with his idea of higher syntheses, is induced to find the corresponding division in the folk-soul. He actually goes so far as to correlate language, religion (*Mythus und Religion*), and custom—the elements of the folk-soul—with the ideas, emotions, and volitions, respectively, of the individual soul. "Die Sprache enthält die allgemeine Form der in dem Volksgeiste lebenden Vorstellungen und die Gesetze ihrer Verknüpfung. Der Mythus birgt den ursprünglichen Inhalt dieser Vorstellungen in seiner Bedingtheit durch Gefühle und Triebe. Die Sitte endlich schliesst die aus diesen Vorstellungen und Trieben entsprungenen allgemeinen Willensrichtungen in sich. . . . So wiederholen sich in Sprache, Mythus und Sitte gleichsam auf einer höheren Stufe die Elemente, aus denen sich der Tatbestand des individuellen Bewusstseins zusammensetzt."[1] The superficiality of this analogy is manifested by the altogether arbitrary selection of the attributes of language, religion, and custom. I can conceive of no reason why mythology and religion, for instance, should not be correlated just as well with the ideas or the volitions as with the emotions. Furthermore, it must be borne in mind that Wundt's tripartite division of culture into language, religion, and custom—a division which is of course convenient for the above analogy—is in itself altogether arbitrary. Why social organization and technology, for example, should not find coördinated divisions is incomprehensible. The correctness of

[1] 'Probl. d. Völkerpsychol.,' 29–30; see also 'Logik,' 3, 232; 'Völkerpsychol.,' I, 1, 26, 27.

this statement is, indeed, demonstrated by the fact that in his latest work on folk-psychology, in his 'Elemente,' Wundt himself ignores the tripartite division in favor of a consideration of all categories of cultural phenomena. The above analogy between the elements of the individual soul and those of the so-called 'folk-soul' is no less crude than the analogy sometimes drawn between the state and a physiological organism on the basis of certain superficial similarities.

VI. The Psychogenesis of the Folk-Soul

In his folk-psychological terminology, Wundt introduces the concept 'social will' (*Gesamtwille*), which, analogous to the concepts 'folk-soul' and 'social mind' (*Gesamtgeist*), corresponds on the social side to the will of the individual. To be sure, the distinction which Wundt makes between the concepts 'folk-soul,' 'social mind,' and 'social will' is frequently far from being clear. He is inclined to use especially the term '*Gesamtwille*' in a way that demonstrates clearly to my mind the line of thought that leads him to the idea of social life as an over-individual synthesis. My point is—and it is probably profitable to state it at this point—that Wundt derives the idea of the folk-soul from the collective actions of the group as an *organized* social entity. At times one even gets the impression that it may be derived in a superficial way from the legally organized state of the present day. For instance, while trying to demonstrate that the reality of the group is of a higher order than that of the individual, Wundt states: "Der praktisch bedeutsamste Beweis scheint mir freilich darin zu liegen, dass die Normen des Rechts nur aus einem realen Gesamtwillen jene verpflichtende Kraft schöpfen können, vermöge deren sie ihre unbedingte Herrschaft über den Einzelwillen behaupten."[1]

It is significant that Wundt cites criminal law as a specific example. A similar specification of the social will in terms of organized society is expressed in the following passage: "Nun findet sich der Wille des Einzelnen eingeschlossen in einer

[1] Wundt, 'Ueber das Verhältnis des Einzelnen zur Gemeinschaft,' *Deutsche Rundschau*, 1891, 203.

Willensgemeinschaft, die mit ihm in fortwährender Wechselwirkung steht, so dass er, vom Gesamtwillen beeinflusst, selber wieder nach Massgabe der erreichten individuellen Entwicklung diesen bestimmt. So ist der Einzelne zunächst Mitglied eines Stammes, einer Familie, einer Berufsgenossenschaft, dann bei sich erweiternder Willensentfaltung Glied einer Nation, eines Staates, um schliesslich mit diesen höheren Willenseinheiten teilzunehmen an einer . . . Willensgemeinschaft der Kulturvölker."[1] Bearing this rendering of the social will in mind, and recalling that language, religion, and custom are, according to Wundt, the creations *par excellence* of the folk-soul, it is fair to infer that these cultural achievements are conceived by Wundt as created by the group as an *organized* social unit. That this is his meaning, is implied in the following statement:

"In der Tat bilden ja Rechtsordnung und Staat nur hoch entwickelte Formen eines gemeinsamen Lebens, das von frühe an in der eine Volks- oder Stammesgemeinschaft verbindenden Sprache, in den ihr eigentümlichen religiösen und mythologischen Anschauungen, endlich in den für alle verbindlichen Normen der Sitte sich äussert."[2]

While from an *a priori* point of view it may seem plausible enough that language, religion, and custom are the evolutionary products of the social group as such, it is easy for modern anthropology to point out that the homogeneity and continuity of development as implied in the psychogenesis of the organized group are not borne out by empirical data. We do not find types of language, of religion, of mythology, of custom grouped in such a way as to justify us in viewing cultural evolution as a single line of development. For instance, we find the Athapascan-speaking Navaho in absolute social isolation from the remote northern Athapascans, and with a culture characteristic of the southwestern area. Again, we find the Tewa-

[1] 'Syst. d. Phil.,' 1, 389. It is interesting to note that in a chapter of his 'System der Philosophie' (2, 188–211) devoted to the evolution of the social mind ('Entwicklungsformen des Gesamtgeistes') Wundt treats exclusively of the forms of social organization beginning with the tribe and leading up to the modern state. Here he ignores all other possibilities of a broader aspect of cultural development, in spite of the fact that the 'Gesamtgeist,' by way of definition, pertains to the psychogenesis of culture at large.

[2] Wundt, in *Deutsche Rundschau*, 1891, 198.

speaking inhabitants of Hano living in the closest social and cultural relations with the Hopi, in spite of the difference of tongue. The Plains area shows a great diversity of speech associated with a great similarity of other cultural factors, especially of material culture. Anthropology furnishes numerous instances of the constant dissociation in the distribution of cultural elements.

Wundt's folk-psychology pretends to be based on empirical facts; but is it not rather an unhistorical construction, inspired by the *a priori* idea of the manifestation of the folk-soul in the organized social group? When I protest against the identification of the folk-soul with the organized group I do not mean to question, of course, the social factor in cultural development. That would be an absurdity. The point lies in another direction. The question is whether we can conceive the development of culture, language, religion, and custom in the form of a single line of psychogenesis, as implied in Wundt's idea of the 'Gesamtwille' and of the constructive development of the social group.

I emphasize the 'single line' of development, because Wundt's psychogenesis[1] represents psychic evolution as a *typical* and universally valid succession of developmental stages. He thus intentionally abstracts from the heterogeneity and multiplicity of the lines of development as they are presented to us empirically in the history of culture. Wundt's idea of psychogenesis is determined by his postulate of the purely psychological, non-historical nature of all folk-psychological problems. The fundamental question is whether such an abstraction from all concrete historical data is methodologically permissible. Quite apart from other theoretical considerations Wundt's method is condemned, in the eyes of the anthropologist, by the fact that, while positing the psychology of cultural development as the particular object of folk-psychology, it ignores completely an account of the inherent schematism of its unilinear construction, the two fundamental psychological problems of cultural development; namely, that of culture areas, and that of cultural diffusion. These two

[1] 'Elem. d. Völkerpsychol.,' 4.

complementary psychological problems transcend the realm of the unilinear psychogenesis of the organized group as such, and thus find no place within the dogmatically limited sphere of Wundt's would-be social will (*Gesamtwille*). What is the psychological significance of cultural specialization within certain geographical areas—a specialization quite distinct from the distribution of language, and in no way limited to social units? What is the psychological interpretation of the relation of cultural centers—those focal points that appear like the crests of waves—to the outlying fringes of the areas? What are the psychological situations that quicken the diffusion of borrowed cultural elements? What are those that retard or exclude the diffusion? What are the factors that determine on the one hand the passive absorption, on the other the active assimilation of borrowed traits? These are all real psychological problems. They apply specifically to those cultural phenomena which Wundt's folk-psychology seeks to comprehend—to such phenomena, for instance, as sound-shifts, the psychical transformation of religious ideas, and the heterogeny of custom. But Wundt's folk-psychology ignores such problems in favor of a construction which, while ingenious, blinds us by its very nature to the real psychological problems as presented by the empirical facts.

I have tried to demonstrate that Wundt derives his concept of the social will from the group as an organized social entity with collective modes of action. This derivation is characteristic of Wundt's attempt to arrive, through a process of conceptualization, at a purely psychological construct, to be operated with in a would-be science of over-individual syntheses. This construct is the folk-soul. Inasmuch as the problems of folk-psychology are to be purely psychological, the folk-soul must be conceived as a psychological actuality *abstracted* from the concrete historical phenomena. But where is this purely psychological something demonstrable, since all cultural developments are given as concrete historical phenomena? It is not difficult to understand why Wundt, in this dilemma, seizes upon the collective actions of the organized group as a tangible realization of the social will, and thus, in

his terminology, of the folk-soul. But one dilemma is not annulled by the addition of a new one. The construction of a typical and unilinear psychogenesis of language, religion, and custom is contradicted, as we have seen, by the culture-historical phenomena themselves, from which the folk-psychological construction is supposed to abstract. The net of contradictions in which Wundt thus becomes involved is caused, as will be shown, by his *a priori* assumption that in the study of cultural phenomena a separation of the psychological from the historical point of view is methodologically feasible.

VII. The "Typical" Nature of Folk-Psychological Inductions

Before entering on the discussion of this final point of our criticism, it is necessary to dwell for a moment on Wundt's proposition that the inductions of folk-psychology are of 'typical'[1] significance. This is a fundamental point for Wundt, inasmuch as it is a direct expression of the 'purely psychological' bearing of folk-psychology, and thus implies his idea of the absolute distinction between psychology on the one hand, and history on the other. According to Wundt, the phenomena of psychology are 'typical'; those of history, 'singular' (*singulär*).[2] Individual psychology deals with the psychical processes of individual consciousness, in so far as they are 'typical.'[3] The processes of folk-psychology as an essential branch of general psychology are characterized in the same way. "Gegenstand einer psychologischen Disziplin kann . . . überall nur das Allgemeingültige, Typische sein. . . ."[4] The concept of the 'typical' in folk-psychology is derived from the corresponding concept in individual psychology by means of Wundt's general analogy of the individual soul as the

[1] I use the term 'typical' to convey the meaning of Wundt's terms 'typisch' and 'allgemeingültig.'

[2] I would call attention to the specific sense in which I use the term 'singular' in order to make it correspond to Wundt's 'singulär.' It is the contrasting term of 'typical.'

[3] 'Logik,' 3, 162.

[4] 'Logik,' 3, 230.

microcosmos, and of the folk-soul as the macrocosmos. This analogy, with its derivative concepts, appears plausible enough as long as it remains in an ethereal sphere of abstractness. As soon as such ideas are elaborated for specific application, they become self-contradictory. That this is precisely what happens to the concept 'typical,' I shall attempt to demonstrate presently by means of certain inconsistencies in Wundt's considerations.

The subject-matter of folk-psychology, language, religion, and custom, is originally given in the form of 'singular' phenomena, and is thus subject priorily to a purely historical consideration. From these 'singular' data of history, however, the 'typical' material of folk-psychology is gained by means of analysis and comparison. The 'typical' something that is thus supposedly found is referred to by Wundt as the 'common attributes of the folk-mind' (*allgemeine Eigenschaften des Volksgeistes*).[1] Then, again, he speaks of the products of the refining-process as the 'universal laws of psychical evolution' (*universelle geistige Entwicklungsgesetze*).[2] In another connection, however, Wundt explicitly states that the general psychical laws that are borne out by folk-psychology are necessarily already completely contained in individual psychology. He says: "Darum ist es von vornherein ausgeschlossen, dass in der Völkerpsychologie irgendwelche allgemeine Gesetze des geistigen Geschehens zum Vorschein kommen, die nicht in den Gesetzen des individuellen Bewusstseins bereits vollständig erhalten sind."[3] By combining these two propositions of Wundt we arrive at the conclusion that the laws of folk-psychology, which are supposed to characterize the attributes of the folk-soul, are not at all characteristic of the folk-soul, inasmuch as they are but applications of individual psychology. The contradiction thus incurred demonstrates the meaninglessness, on the one hand, of the term 'typical' in folk-psychology and, on the other, of the juxtaposition of the individual-soul and the folk-soul.

1 'Probl. d. Völkerpsychol.,' 28.
2 'Probl. d. Völkerpsychol.,' 24; see also 'Logik,' 3, 240.
3 'Logik,' 3, 227.

There is another point of view from which the "typical" nature of Wundt's folk-psychology can be criticised. As we have seen, its 'typical' significance implies theoretically that the subject-matter of folk-psychology is 'purely psychological'; that is to say, non-historical. In order to ascertain what this 'typical,' non-historical something is, we naturally turn to Wundt's work on folk-psychology itself. What we find here differs strikingly from what the theoretical foundations have led us to anticipate. The three spheres of folk-psychological investigation are, according to Wundt, the purely psychological problems of language, religion, and custom.[1] Those of language and religion are dealt with in his large work on the subject; those of custom, in a part of his 'Ethik.' A glance at the contents suffices to show that in reality Wundt's folk-psychology, far from dealing with 'purely psychological' problems, consists of psychological interpretations of a hypothetical *historical* construction. The content is certainly not non-historical: it differs only from the usual conception of history, in that Wundt has replaced the empirical account of historical developments by an historical construction which he regards hypothetically as the general course of the development of all language, religion, and custom. The 'purely psychological' of Wundt's theoretical postulates reduces and transforms itself in the actualized product into a 'generalized historical.' Thus the unscientific character of the 'typical' attributes of the folk-soul is proved by the very process of their elaboration.

The point just made is borne out still more clearly in the case of Wundt's latest work on folk-psychology, his 'Elemente der Völkerpsychologie.' Here we have before us a constructive history of human culture embracing the past, present, and the future—a brilliantly worked out scheme of the development of mankind through a number of successive cultural stages. This historical framework is constructed mosaic-fashion by fitting the traits of different cultures together, and by letting the presumably higher forms succeed the presumably lower ones. The selection of traits necessary

[1] 'Völkerpsychol.' (1st ed.), I, I, 24.

for building up in this way a ladder of evolutionary stages characterizes the ensuing edifice as purely hypothetical. What objective criteria, indeed, have we for determining one culture as 'higher' than another—for placing the Australian totemism, for instance, on a higher level than the types of 'primitive man' which Wundt adduces? From the empirical point of view, cultures are not differentiated quantitatively by varying degrees of development, but rather by the qualitative heterogeneity of their psychic specialization. The idea of 'degrees' of evolution can only be determined by an extraneous code of evaluation. In Wundt's case this code is clearly supplied by his purely ethical norm of the development towards a humanitarian ideal ('Entwicklung zur Humanität'), as elaborated in his 'Ethik.' In his 'Elemente,' this normative conception is, as already discernible from the table of contents, responsible for the arrangement of the empirical data of culture-history in a preconceived order of hypothetical stages.

Wundt's 'Elemente der Völkerpsychologie,' we repeat, is an historical construction. That Wundt himself conceives this work as historical is demonstrated by its sub-title, 'Grundlinien einer psychologischen Entwicklungsgeschichte der Menschheit.' But, according to the explicit theoretical foundations, the *raison d'être* of folk-psychology rests on the purely psychological, non-historical nature of its problems. Now an historical subject-matter evidently does not become purely psychological and non-historical, nor does it become 'typical' in Wundt's sense by forcing it into the mould of an hypothetical construction. It is generalized history, and Wundt has in the sub-title named the child by its right name; but by doing that his theoretical foundations of folk-psychology negate themselves. The 'Elemente' prove to be the *reductio ad absurdum* of the science of the folk-soul.

VIII. Psychology and History

The incompatibility of the theoretical foundations of folk-psychology and their actualization—the gap, in short, between the word and the deed—centers about Wundt's failure to apperceive clearly one fundamental problem. This problem,

as I conceive it, is that of the relation of history to psychology. We have already discussed his notion of the 'singular'-historical and the 'typical'-folkpsychological. Wundt does not always give the same connotations to these concepts. One mode of using them has been discussed above. Let us for a moment dwell on a variant form.

Wundt's large work on folk-psychology deals exclusively with what is commonly known as primitive culture, in contradistinction to the culture of documentary history. Wundt motivates the limitation of the scope of his investigations by stating that the early development alone of language, religion, and custom exhibit a *typical* psychogenesis, not yet vitiated by the conscious actions of individuals. These early stages of development are, according to Wundt, common to all peoples, because they are determined by universal psychological motives.[1] In the later stages single individuals become in an increasing degree the determining factors;[2] then the phenomena are no longer 'typical,' they are 'singular.' The typical phenomena of the early stages present psychological problems, and are therefore the domain of folk-psychology. The phenomena of the later stages, due to their 'singularity,' are non-psychological, and thus pertain to history. Wundt expresses this idea by saying that the field of history begins where that of folk-psychology ends;[3] but in his later work, the 'Elemente,' Wundt tacitly ignores altogether this distinction between the earlier and later stages of culture. In his 'Elemente,' as we have seen, he presents a construction of the whole cultural development of mankind, barring the above-said limitation. In fact, the fourth and last stage in this book, called 'die Entwicklung zur Humanität,' is conceived by Wundt as a period which we have by no means completed at the present day.

The distinction Wundt makes in his large work between psychology and history is of dubious scientific value, and is refuted by the contents of his later work. The notion that history applies to phenomena of individual making, in contra-

[1] 'Probl. d. Völkerpsychol.,' 22.

[2] 'Ethik,' 3d ed., 2, 364.

[3] 'Völkerpsychol.' (1st ed.), 1, 1, 25.

distinction to the psychological creations of the folk-soul, is arbitrary and implies a superficial conceptualization of history. The historical phenomena of conscious individual origin are in no way *essentially* distinct from the development of language, religion, and custom. Both groups of phenomena are historical, and both presuppose the psychological setting of culture. This bears directly on the criticism I made on a previous page of Wundt's theory on the relation of the individual to the group. The difference between the individual creations and the phenomena of language, religion, custom is not given objectively in the objects themselves, as would have to be the case if 'history began where folk-psychology ended'; but the difference is rather determined by the angle from which we ourselves view the objects. Let us take an example. Wundt states that the history of literature is the successor of the psychology of language.[1] The former deals with the historical creations of individuals; the latter, with the psychological genesis of the folk-soul. This distinction is plainly unmethodological, because Wundt contrasts and brings into an identical line of development two fields of research that are not comparable on account of the different subjective points of view adopted by the scholar, in spite of the common historical nature of the empirical objects. The psychology of language does not *develop into* the history of literature, as Wundt would have it. It is rather the focus of our interests that shifts.

A similar confusion of the relation of psychology to history is demonstrated by the way in which Wundt delineates the difference between ethnology and folk-psychology. In the relation of these two sciences Wundt rescues the 'purely psychological' (!) nature of folk-psychology by defining ethnology as a genealogy of peoples. Its problems are not psychological. Wundt says, "Die Ethnologie ist eine Wissenschaft von der Entstehung der Völker, ihren Eigenschaften und ihrer Verbreitung über die Erde. . . . Hier können scheinbar kleine Kunsterzeugnisse und ihre Abänderungen in hohem Grade bedeutsam sein für die Feststellung einstiger Wanderungen, Mischungen oder Uebertragungen."[2] These migrations, mix-

[1] 'Völkerpsychol.' (1st ed.), 1, 1, 25.

[2] 'Elem. d. Völkerpsychol.,' 5.

tures, and borrowings associated with the repression of psychology savor suspiciously of Graebnerian diffusionism, which this ethnologist has thought well to formulate as 'the method of ethnology.' It seems probable that this 'Kulturkreislehre' has been fatal to Wundt's conception of ethnology.

Enough has been said, I think, to show that Wundt has failed to bring psychology and history into an harmonious relation with each other. This is proved by the mutual contradictions of the theoretical foundations of folk-psychology, as well as by the gap between these foundations and their actualization. Graebner pretends to solve the problem of the relation of psychology to history by ignoring it. For him psychology does not exist, and history is something that serves as a bait for his 'Kulturkreislehre.' Wundt sees the reality of the problem, and answers it by drawing a sharp line between history and psychology. With this distinction the *raison d'être* of folk-psychology stands and falls. The impossibility of this distinction leads to the self-negation of folk-psychology in Wundt's 'Elemente.'

A criticism of Wundt's folk-psychology and of its theoretical foundations has a deeper bearing than the mere fact of pointing out logical discrepancies. The significant fact is that in the case of Wundt's folk-psychology a most ingenious attempt to mark out clearly a distinction between psychology and history has failed. Wundt has devised a remarkable foundation of concepts upon which to build up a new science of the folk-soul. His concepts of the higher synthesis, the social mind, the reality of folk-psychological actuality, etc., are all seemingly firmly anchored in a monumental philosophical system; but Wundt's conceptual scheme breaks down when applied. His failure is significant, since it proves the inconsistency of drawing a line between history and psychology. That history without psychology is an impossibility is proved by Graebner; that a non-historical psychology of culture, a folk-psychology, is likewise a misconception is proved by Wundt. It would seem to me that history, when taken in its broad sense as the history of culture, is intrinsically associated with a psychological point of view. The relation of psychology to history

is much the same as that of physics to physiology. Historical phenomena are interpreted psychologically as physiological processes are interpreted in terms of physics. The general scepticism that this form of the relation of psychology to history encounters is due, I believe, to two causes; firstly, to the disreputable rôle that popular psychology has played; and, secondly, to the unaccustomed novelty of thinking of history in its broadest possible sense as the history of culture (*Kulturgeschichte*).

What an intrinsic association of psychology and history can attain is well exemplified by numerous individual passages in Wundt's works on folk-psychology, when we abstract from all his theoretical foundations. There we find psychological interpretations of historical phenomena executed with a brilliancy characteristic of Wundt's genius. Such interpretations of Wundt will mark the monumental significance of his work long after folk-psychology as such will have been recognized as an 'Unding.'

ATTITUDE AS IT AFFECTS PERFORMANCE OF TESTS

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The aim of mental testing for ability is certainly the ascertainment of the individual's potential; it properly is nothing else, and if there are factors which interfere with such ascertainment they must be recognized and allowed for. Experience has taught us the great importance of one factor—we have learned that performance of psychological tests is vastly influenced by mental attitude.

Insofar as mental attitude has been discussed at all in psychological literature it has been treated mainly as a factor in the learning process. In his 'Educational Psychology' Thorndike has enumerated the law of attitude as one of the five subsidiary laws of learning. He says: "It is a general law of behavior that the response to any external situation is dependent upon the condition of the man as well as upon the nature of the situation." And again: "Only a little less obvious should be the fact that the attitude or set of the person decides not only what he will do and think, but also what he will be satisfied and annoyed by." In another chapter he states that, "Purposive behavior is the most important case of the influence of attitude."

Ruger, Book, and Swift, in their experiments on the learning process, give the introspective accounts of various subjects as to the influence of attitude. Ruger states that the attitude of confidence was a great aid in the successful solving of puzzles; Book speaks of the influence of a favorable "feeling-tone"; and Swift mentions the ill effects of a feeling of discouragement on the part of the learner. Aside from these general discussions little has been written on the subject.

Just as in the learning process, so in the performance of tests for ability the influence of attitude is all potent. The

mental set with which a person approaches a task is a large factor in success or failure. Mental attitude has very wide bearings; it is present at all times and influences all behavior, but in no realm is it more important than in the performance of psychological tests.

Though most observers recognize in a general way the importance of this problem, no experimental effort has ever been made, so far as the writer knows, to measure just how strong a force attitude is; nor would it be an easy task. To study it by introspective means would be possible only with skilled subjects, and to deal with the problem objectively would be possible only by creating very artificial situations whose influence even then might not be the same with any two individuals. In group or class experiments it is almost impossible to gain an insight into the attitude of the various members. It is only when individuals are being tested singly that opportunity arises to study this question.

And yet, although so little has been done, the problem is an exceedingly important one. Today, when so many practical issues are met on the basis of the diagnosis of the individual's mentality, when schools and courts are dealing with children and adults according to the findings of the psychological laboratory, all related problems become vital. Sometimes it would seem as if the examiner is content to determine the subject's ability without regard to any and all kinds of hampering conditions. But when we consider that the entire future care and education of an individual may depend on the verdict of the examiner, we realize the importance of weighing each reaction in the light of all the conditioning factors.

In our work in connection with the court we have unusual opportunities for studying the problem of attitude and for evaluation of it as a factor. It is true this can be done only in descriptive, not in quantitative terms; but because the situations are so real the proof becomes very clear and therefore valuable. We have situations of real life, complex as those really are, and yet controlled in a measure as they may be in any laboratory.

From observations of these living conditions we know that

attitude affects practically everything, and in turn it is affected by well-nigh everything. All external features of a situation influence it. Thus the psychological laboratory must be a quiet room, free from distraction, with good light and ventilation. Then there is the more subtle influence of the personality of the examiner, whose peculiarities and mannerisms are factors more often than we realize. One need only mention the influence due to the presence of onlookers at the time of examination to realize the serious consequences on the attitude of the examinee and the frequent vitiation of test results caused thereby.

The time and place of examination too are matters of vital importance; in work such as ours, where the psychological laboratory is a department of the court, this is especially true. To examine a person at the court, either immediately before or after his appearance before the judge, is the greatest folly, for the results are often unreliable because of the disturbed mental condition. Equally as foolish is the examination soon after an order of commitment to an institution has been made. In these cases there are injected into the situation elements that may affect the results to such an extent that the picture presented is utterly false.

Further, there must be sufficient time for each examination to permit of a natural introduction to the test. There must be some opportunity for presenting a motive for exerting one's best efforts. Dewey has shown that motive is always a large factor in successful achievement; this applies equally in the performance of tests. Not infrequently we are asked the purpose of the testing and we know, in the light of our experience, that the giving of a motive that seems rational means coöperation on the part of the subject.

Sometimes the difficulty is just the reverse. Before the individual comes to us he has been given an explanation by others which leads to an unfavorable attitude. For example, some one has told him that the purpose of the testing is to determine whether or not he is insane or feeble-minded. In such cases, where there is apprehension because of this, the attitude must somehow be changed before beginning the examination if we are to reach conclusions that are valid.

Our practical procedure endeavors to meet these conditions: The young person has committed an offense; the circumstances are to be investigated and he is to be judged, but he meets us as friends who are anxious to understand and help him. We do not study our cases at court, but while the offenders are in the Detention Home. We can thus wait until they have become accustomed to their new environment, until their first emotions have been mitigated. We have sufficient time for each examination to enable us to make any approach we feel is advisable in the given case, even if it necessitates first becoming friends in the playroom. Because the young person is made to realize that the outcome of his work will be a factor in determining his future career, he has a genuine motive for coöperating and doing his best. We are able to see him again and again, under varying conditions, and to note the differences in responses when these can be due only to difference in attitude. No other element in the situation is altered and hence no other can be offered in explanation of the change. Just because of the complex nature of the situation it is possible for many different attitudes to be observed.

Of course there are frequently instances where retesting does not alter the results, where the first results represent the best the individual is capable of doing. This should confirm our general thesis, since it shows that when failures are due to lack of innate ability the failures are repeated. When on retrial the individual succeeds where he at first failed, there must be some other explanation.

Thus in the case of a boy of seventeen (Case I), the first examination gave such peculiar results that one felt the explanation must be that the lad was displaying a peculiar attitude, probably incriminatory depression. No recalcitrancy was evident, but the boy's mind seemed to refuse to work. He succeeded with things that could be done automatically, but where any real thought was required peculiar inhibitions appeared. He seemed much depressed and exceedingly nervous; while endeavoring to coöperate he would stop work and sit as if pondering about something. Results on tests were very variable and showed numerous peculiarities. Since this

was the boy's first offense we felt that perhaps because of fear and anxiety he was unable to do himself justice, and that it would be only fair to retest him at another time. Less than a month later, after the boy had been released to his parents, was living at home, and could not possibly be fearful of the outcome of his case, he was retested. The results were very much the same as at the first examination. Up to the present time this boy has been seen on three different occasions. We have studied him very carefully and have been unable to find any marked change in his reactions. We have come to the conclusion that the boy is suffering from a psychosis and that the peculiarities in his behavior, when originally seen, were not due to attitude as we had suspected. But before reaching our final conclusions the boy was given every possible chance of doing the best of which he was capable.

We would also emphasize the point that the situation is quite different where the examination deals with adults or older adolescents. Our work, though mainly confined to younger adolescents, has given us ample opportunity of proving this. A young person is more naïve, less self-conscious; the older person, on the contrary, is often embarrassed, self-conscious, and afraid that he cannot do this or that well. He feels either that the tests are insultingly simple, or that they are only apparently so, while in reality meant to "catch him." If he adopts the first point of view he does not coöperate well, and if he takes the latter view he is over-cautious. We have noted frequently that when adults, sent us by our pension department, are being tested they are ill at ease; they frequently state that they feel ashamed that they have forgotten many things which they once knew. The tests are novel to them and there is great difficulty in getting them to adjust themselves to this new situation.

Particularly is it difficult to introduce Binet tests in any natural or interesting way. We frequently hear well-educated people state that they too would fail on such tests; and perhaps they would, simply through fear that their natural response would be too simple to be correct. Doubtless, this is largely the explanation of the peculiar and interesting results on the

Healy pictorial completion test where the younger people whom we see are compared with students at Wellesley College. The fact that these college students did not do as well as our normal but not nearly so well educated group, is no doubt due to the difference in attitude.

As one would expect, it is the emotional aspects that are most frequently disturbing elements. Shand, in his book called 'The Foundations of Character,' emphasizes the important rôle which the emotions play in conduct. Thorndike comments upon the influence of emotions, but in relation to the improvement of any function. What he says, however, holds good in the problem of performance in general. He states it as his belief that the absence of irrelevant emotional excitement and the absence of worry are two conditions conducive to improvement. He believes that all emotional excitement is distracting and is a waste of energy and a preventive of good results.

Certainly, in the performance of tests the emotions often enter in and affect results markedly. We propose to show by means of illustrative cases a number of instances where this is true. Fear, anger, general depression, humiliation, embarrassment, timidity—these and numerous others can be shown to be factors of great consequence.

We here merely mention, in passing, the fact that some physical conditions interfere with performance. We shall not go into detail nor illustrate the peculiar results due to the mental state of the epileptic and hysterical, the reactions of psychoses, major and minor, the mental disturbance of chorea and the effects of bad physical conditions and debilitating habits. No one should doubt the influence of all these upon mental states and thus on responses to tests.

We turn now to illustrative cases representing examples of various types of attitude that we have observed in our work:

I. *Deliberate Deception.*—It is easily conceivable that a person may, from various motives, fail to achieve success willfully and intentionally. He may do so as a matter of resentment or indifference, or because he thinks it to his

advantage. He may believe that if considered incompetent he will be excused from work which he dislikes, or be dismissed from a school or institution where he does not wish to remain. Again, he may have some other reason, distorted perhaps, but logical to him. The first case presented illustrates this last point.

Case II.—A boy, 10 years 3 months, was brought for examination. It was a disciplinary problem, the parents not being able to control the lad. He had been a great truant previously, and in consequence had been sent away from home to a school for truants where he had remained for some months.

He graded 6⅗ years by the Binet tests,[1] failing on all the 8- and 9-year groups. He failed on the simple test for mental analysis, namely, the crossline test,[2] said $8 + 4 = 13$; $4 + 6 + 5 = 16$; altogether a very stupid performance. Asked to read a passage beginning, "It was autumn, the leaves were falling and the cold winds had begun to blow," he said, "It was autumn, the leaves were *not* falling and the cold winds had *not* begun to blow," etc.

The boy was plainly told that if he could do no better he would have to be sent away to a school for stupid children and that perhaps he was so bad because he was so dull. His face expressed the greatest amazement and he immediately said he could do better. On retesting him, he not only passed all the 8- and 9-year Binet tests correctly, but all the 10-year tests as well. He added, subtracted, and multiplied fairly difficult problems without error and proved capable of solving a number of performance tests. Asked later to explain his queer behavior, the boy gave the following facts: Before being sent to the school for truants he had been examined at another clinic; he had done his best and had been sent away. Now, remembering his past experience and not wishing to be sent away, he thought that by doing poorly he would escape his

[1] Throughout this article Binet tests refer to the last revision by Binet himself, namely, the 1911 series.

[2] See Healy and Fernald, 'Tests for Practical Mental Classification,' Psychological Monographs, No. 54, March, 1911.

former fate and determined to fail. Good logic, as one sees, if only his premise had been correct.

In this case the willful deception, so easily understood when explained, might not have been discovered if no attempt at analysis had been made. If the examination had been made hurriedly and the examiner had been content with the objective first findings, an egregiously false judgment might have been formed. Incidentally, this case illustrates too the value of obtaining former history to corroborate a diagnosis.

Case III.—A very similar situation is presented in the case of a little girl 12 years and 4 months, reported by the school authorities, by the mother, and by a probation officer as being "exceedingly incorrigible." The principal of the school brought the girl to us because he thought she must be mentally defective; indeed, she had already been placed in the subnormal room. She was said to be exceedingly willful, obstinate, and bad tempered; she was besides a great truant.

When we first studied the case we found this little girl graded $9\frac{3}{5}$ years by Binet tests and succeeded with just about third-grade school work. Thus she was apparently sufficiently retarded to come within the group of those denominated as feeble-minded. Because of her general bad behavior this girl was sent to an institution for semi-delinquents. There it was reported that she was feeble-minded, that she could not do any school work, nor was she educable. Wondering if the girl had done much worse on testing than she might have except for her unfortunate attitude, we retested her just three months after the first examination. We found that she passed all 10- and 12-year Binet tests readily, and that she did a number of other tests quite well for a girl of her age, grading thus slightly higher mentally than her chronological age. At this time she confessed that when first seen she had purposely done poorly, thinking that she would then not be held accountable for her bad behavior; that she had pretended to be exceedingly stupid hoping that they would refuse to keep her there. Now the discipline of being sent away from home, which was very distasteful to her, had proven so effective that she was most anxious to do her best. Later reports have shown that this

girl is quite capable. She has been returned to the public school and all reports have been satisfactory.

Case IV.—The next case of purposive failure presents an instance where the boy thought that by proving himself inefficient he would be dismissed from the institution to which he had been sent. He had been told by other boys that he would be kept there indefinitely if he had a good record, but that bad and stupid boys were allowed to return home. Examined there, the verdict had been that this boy of 15 was mentally a high-grade moron, $10\frac{4}{5}$ years by Binet, that he was erratic, his memory power and judgment defective. On the strength of this diagnosis was incurred all the expense and trouble of sending him to the city as a candidate for the school for the feeble-minded.

When we first saw the boy he was anything but friendly, but he passed, nevertheless, all the Binet tests up through the 12-year group, except one of the latter. He did construction tests fairly well but failed on the second crossline test even after four trials. Nothing further was attempted in the way of tests that day, but an effort was made to gain the boy's interest and good will. Retested the next day, when his attitude was much better, he succeeded on the failed 12-year Binet test, and passed four of the 15-year tests, was correct on the crossline, first trial, did some fairly difficult work in fractions, and learned a new process in multiplication of fractions. Repetition of this later proved that the boy had grasped the principle involved and that he was quite capable of further education. He frankly said to us this day that when tested at the institution he had "stalled," and explained the motive for his behavior as given above.

II. *Recalcitrancy.*—Often, however, it is a matter of recalcitrancy rather than purposive deception which explains the mental attitude of the subject. One example will suffice to depict a typical case, though this might be duplicated indefinitely if there were need. The point can be so readily seen that repetition of cases is unnecessary.

Case V.—Immediately after seeing this boy for the first time, the examiner dictated the following impression: "He turns

his head away, jerks his shoulders, says, 'I can't do it,' 'I don't know.' He is stubborn, yet clever in shirking work. Thus he said that he did not know the difference between various objects; that he only knew their Polish names. (He later did splendidly on this test.) He succeeded on numerous of the tests only after both cajolery and scolding were tried. Furthermore, the teacher in whose class he is says of him: 'He is a bad boy. He hits others and then is vociferous in his denial of having done anything wrong. He is a typical bad boy.' The officer says, 'He is as shrewd as a little fox.'"

We note that the home conditions might well account for a part of this boy's clearly shown tendencies, for the father is reputed to be 'a brute, immoral, many times arrested.' The mother is in the last stages of consumption and exerts little control over the children.

Had this boy been judged by the actual results of his first tests he would have been considered feeble-minded. Realizing that these did not present a true picture of innate ability an effort was made to change the boy's attitude, with sufficient success to cause a vast amount of improvement in the final outcome on the second testing. Thus after the first morning's work he graded 8 years by Binet; three days later this was raised to $9\frac{3}{5}$ years. He failed on the simple crossline test on the first day's efforts, later doing this correctly at once. When first seen he was unable to do—or claimed he was unable to do—the simplest addition, such as $2 + 2$, whereas finally he did ordinarily difficult problems in addition and subtraction. His record on construction tests improved vastly, but of course part of this may have been due to practice effect. However, it is clear that the tests which originally had to be scored as failures were successfully solved when the boy was actually coöperating.

No motives for this boy's earlier reactions were ever discovered; they seemed to be typical of his generally recalcitrant behavior.

III. *Sportiveness.*—There are persons who are not inclined to be serious about anything; they accept all life somewhat as a

joke and their 'devil-may-care' attitude may often be one of the causes of their delinquency. They may show this same attitude towards testing; indeed, in the example cited below, the boy acknowledged that when first tested at the correctional institution he had made no effort to succeed, but rather had quite enjoyed the fact that his simulation of disability had been accepted seriously by the examiner. His attitude, according to his own words, was "I put it over on the doctor." That the outcome might seriously affect his whole future had not occurred to him, nor had any one made this plain to him; the testing was just another opportunity for fun. He did not interpret the situation as anything of serious import.

Case VI.—This boy, 16 years 8 months, was quite troublesome at the institution, and since the result of the examination there gave an opportunity for transferring him to the state school for the feeble-minded, it was desired to have the court order this done. Thus we were asked to study the case.

The report sent with him was that he was a middle-grade moron, 10⅘ years of age mentally. The superintendent, among other facts, stated that the boy was noted for his attitude of indifference.

With us he at once inquired why he was asked to 'do these things.' He wished to know if we were determining if he was 'crazy.' We explained that there was no question as to his sanity, introducing the tests on a vocational basis and trying to make him realize the vital importance of doing his best.

He proved conclusively that he was quite normal in ability, for he succeeded easily on all the 12-year and three of the 15-year Binet tests; did both construction and both crossline tests well; showed good apperceptive ability—in fact did quite well on a wide range of tests, the only exception being on formal school work; this was due partly to his earlier truancy, partly to early bad environmental conditions, and possibly somewhat to his general attitude. The boy himself acknowledged that his efforts with us had been more serious; he said with quite a little glee, for example, that previously, while the examiner was looking out of the window, he had mixed up the Binet weights. Certainly we proved that this lad could not be

regarded as mentally defective and the earlier recommendation, namely, that 'if some guardian will not be responsible for him he should be sent to an institution for the feeble-minded,' was one with which we could not agree.

IV. *Emotional Disturbances.*—Any one of a number of causes may lead to such emotional disturbance on the part of the examinee as will affect the test work greatly. We can readily appreciate, on the basis of personal experience, that emotional distress may cause great lack of self-control and inability to center attention on the task in hand. It may cause one to adopt an attitude decidedly disadvantageous for the performance of any kind of mental work.

Proceeding from our experience with results on tests quite different from what they otherwise might have been, we subdivide the causes according to the emotional conditions conducing them. They appear as follows:

(*a*) *General Depression: Case VII.*—In the first case cited, the examiner, immediately after testing the boy, dictated the following: "This boy's manner throughout the tests was peculiar. He seemed not so much nervous as distraught; for long periods of time, perhaps as much as a minute or two, he sat doing nothing. There were other long time reactions in a number of the tests. In spite of the fact that the Binet tests were well done, except for some peculiarities, and all school work was satisfactorily accomplished, the results on many other tests were decidedly poor. The impression made is that the boy is not incapable so much as extraordinarily peculiar."

When the results on tests during this first interview are noted in detail, the decided peculiarities of this big, strong boy of 15 years 6 months are seen at once. He tells us that he has reached the seventh grade. The simpler construction test, readily performed by younger children, he fails to accomplish in 5 minutes. During all this time the boy made but 13 moves, the first 3 of which were done in 3′. The more difficult construction test is a failure at the end of 10′, during which time only 13 moves had been made. Although he succeeds with the simpler crossline test on the first trial, he

fails on the second one after four trials. The pictorial completion test is completed in 8′ 48″ with 7 errors, 3 of which are illogical. The easy opposites test shows an exceedingly irregular time reaction, the range being 1.4″ to 9.2″, the average time 3.5″; three reactions are scored as failures because of being given after 10″ had elapsed. In the Kraepelin test, where the numeral 7 is continuously subtracted from 100, several steps are done very rapidly; then a long pause occurs, but the whole is completed in 1′ 41″ with only 1 error. Taken by themselves the results give a picture of reactions which might lead one to believe the boy to be the victim of a psychosis.

Later, in conversation with this boy, it was found that as soon as his story was approached he became much frightened; beads of perspiration stood out on his forehead, and big and strong as he was he began to cry. In the course of his story it was discovered that this was the boy's first arrest and he acknowledged that he was exceedingly unhappy; that he was continually thinking of his mother and that he was unable to keep his mind on what was being asked of him.

Several days later, after the family had visited him and there had been a discussion during which it was virtually promised that the boy would be allowed to go home on probation, he was asked to try again a few of the tests. He now did the first construction test in 16″ with 6 moves; the second construction test was performed in 40″ and 11 moves, an extremely good record. The second crossline test was correct on the first trial. The completion test was performed in 1′ 30″ with only 1 illogical error. The opposites test was accomplished without error or failure and with quick and even time reaction.

Nothing could be clearer than the fact that this boy originally was laboring under such mental disturbance as to make it utterly impossible to test him fairly under the circumstances. Just such states of mind, no doubt, frequently occur, but the examiner does not always take the trouble to learn just wherein the difficulty lies. Or he may not have considered the matter enough to be conscious of the important rôle it plays.

To illustrate that even such an apparently small matter

as the propinquity of a relative may serve to spoil a performance, we present briefly the next case:

Case VIII.—A boy was brought to the laboratory for testing; unfortunately on the way he discovered that his mother was in the building. When he began his work it was seen that the time was not propitious, for his emotional disturbance was quite apparent. He failed on the first construction test and performed the completion test with 4 errors. Of course, there was nothing to do in this case but to defer the examination until another time. Comparing the results of the two tests mentioned above with the results attained later, we find that the construction test was correctly solved in 2′ 30″, and the completion test performed with but 1 error when the boy was seen under more favorable circumstances.

Case IX.—A third case is cited because so much time was given it and because the verdict was exceedingly important. A boy of 16 was studied at a reformatory institution where it was recommended he be transferred to the institution for the feeble-minded. The report of this examination was that the boy's mental age was $10\frac{1}{5}$ years; that he was indifferent, mentally suffering from delusions grafted on feeble-mindedness. Before this transfer was made the boy was sent to us for further study.

We found him greatly depressed. Not only that, but the boy himself had a distinct grudge-attitude. He felt that he had not been justly treated in being sent away, and indeed, when all the circumstances were known one was of the opinion that the boy was a victim of many bad circumstances rather than altogether the culprit. He came from a miserable home. The mother was immoral and at the time was serving a sentence in a correctional institution. All the circumstances regarding this were known by the boy and he was very unhappy about the whole matter. During his stay at the institution to whch he had been sent he was discontented; had run away once and been sent back.

A great effort was made to have the boy feel at home with us and to realize that we wished to be of assistance to him. Although he never became extremely friendly yet he changed

his attitude sufficiently to make a greater effort in the direction of tests when it was made plain to him how largely his future welfare depended upon the outcome of his work. We found that he was able to pass all the 12-year Binet tests and three of the 15-year tests. He made an extremely good record on our construction tests, succeeded on both the crosslines with one trial, showed quick and normal apperceptions on the completion test; in fact, the results were uniformly good, except on tests which required good powers of concentration and mental control, such as the opposites and the Kraepelin. In consequence we were led to believe that the boy had not done himself justice when tested at the reform school, very largely because of his general depression and his unwillingness to coöperate as well as he might have. His so-called delusions were really well-founded feelings concerning his inability to make good when he should be sent back to his old environmental conditions.

Of course there are instances of depression verging on despondency or melancholy. Illustrations, however, would lead us into the field of the psychoses, which we are not discussing here. We must recognize clearly, however, the fact that even in cases of depression, such as we see, there is often almost a typical paralysis of thought. The repugnance for mental effort, so characteristic in melancholics, is evident, though in lesser degree, in cases where the individual is unhappy or extremely anxious.

(*b*) *Anger or Resentfulness*.—This may exhibit itself in two different phases; it may be an attitude or feeling entertained towards some other person, or it may be anger at the world in general, oneself included.

Case X.—The first type is illustrated in the case of a girl of fourteen whom we were asked to see. We have only a single record on tests but one is confident that the results are unfair, not because we have proof of this by later testing, as in the cases cited above, but because the general attitude of the subject was so perfectly apparent. When brought to the laboratory she at first refused to do anything; maintained that there was no need to do anything for her; she wished to be

sent to a reformatory. She showed at once that she was self-willed and moody. When on one or two tests she became interested it was as if this were against her own will. She was indifferent to commendation, uncommunicative, and showed the same characteristics toward several people who tried to convince her that they wished to be her friends. The notable point may be added that, as frequently occurs, the girl did performance tests when she absolutely refused to attempt any other kinds. Often the examinee will coöperate on tests with concrete material when he fails on anything in which he has either to speak or to deal with abstractions.

The construction tests were fairly well performed in spite of her indifference, but her failure to succeed on the second crossline test, even with four trials, was undoubtedly due to her attitude and not to her incapacity. She refused to attempt several of the tests; on the testimony experiment she gave but three details and said she had seen nothing more in the picture. It can readily be seen how well-nigh hopeless it is to make any diagnosis of such a case when but one interview is possible. If one accepted the girl's statement, "I can't do it," as being literally true, rather than as indicating "I won't do it," one would certainly do the girl a great injustice, for from her general conversation it was apparent that this girl was at least normal in ability. She was, however, furiously angry at the probation officer for bringing her into court, and in consequence would coöperate with no one.

Case XI.—The extreme instance of this type of behavior is seen in the case of a girl of 16, who talked very well and was said by the teacher in the school-room to do seventh-grade work well and easily. When brought to the laboratory this girl steadily refused to do any tests. It was reported by her mother that often she exhibited great obstinancy, had a very bad temper, and that she was very loath to exert herself in anything. The whole case is summed up in the girl's own words: "I am not weak; I just do as I want to. I don't intend to do any puzzles for you because Mrs. A. (an attendant at the Home) said, when she was mad, that she was glad I was going to have my brain examined. I'll not do any more work

for you; if you make me, I'll do it all wrong." We have often noticed a similar attitude of resentment evinced by those whom we are asked to see after they have been committed by the court to some institution. This has been borne in upon us so strongly that we have reached the conclusion that it is unfair to see cases just after the court decision has been made. One case will suffice to illustrate this type of reaction.

Case XII.—The first tests were given before this girl went to court, with results that were uniformly good. Because a report was made by the officer that the girl was considered very dull we later asked her to do for us a few more tests, including the Binet. We found her attitude entirely changed. Although she gave answers to questions asked her, we found her resentful, unresponsive, indifferent, and sulky. During the testing, at various intervals, she herself said it was all foolishness. She was going to be sent away and she didn't see why she should do these puzzles anyhow. In consequence the Binet tests, where there was 1 failure in the 9-year, 4 failures in the 10-year, and all failures in the 12-year series, gave us a picture that was certainly not fair.

Case XIII.—The second type of anger is seen in the case of a boy not yet 13 years old. This lad is not normal in self-control. He has spells of excessive anger when he seems more furious with himself than with anyone else. He himself says, "Everything makes me mad." This general attitude is as clearly evidenced by his reactions during the test period as in his general behavior. He begins in a friendly, acquiescent manner, but as soon as his efforts meet with poor success his expression changes to one of intense moroseness and he refuses to make any further response.

He grades as normal in ability, passing all Binet tests through the twelve-year series except one of the last group. After having some slight difficulty in the solving of a test in which he finally succeeds, he becomes so provoked that he fails altogether on the succeeding tests, namely, the more complex construction and the more difficult crossline test. Results continue to become worse and worse. He shows extremely poor apperceptive ability, making illogical errors

in seven of the ten spaces on the pictorial completion test. After a miserable failure on the Kraepelin continuous subtraction test he refuses to try again and then begins to cry.

Seen a few days later he begins very well. He solves correctly, on the first trial, the construction and crossline tests, previously failures, but after making another poor attempt on the apperception test he once more grows angry and will not exert himself further.

This boy's behavior in his own home is in entire consonance with his behavior in the laboratory. He is given to spells of uncontrolled and violent anger. These reactions must be taken into account in forming an estimate of the boy's abilities as well as of his characteristics. There is no doubt that he could achieve success if he could but control himself sufficiently to rationally continue his efforts.

(*c*) *Fear.*—It can readily be seen that many a subject, either not realizing the exact purport of what is being done, or fearful of the action that will be taken as the outcome thereof, will be frightened at what is before him. This may act as an inhibitory force during tests. Realizing this fact we make every effort to prevent the child's being apprehensive, trying to encourage him and make him feel at home. In some instances we simply make no attempt to do any psychological testing at the first interview. Were it not that we are ever watchful of such conditions, no doubt we would find many more such subjects than we do.

Case XIV.—Recently a small boy of 12 was in court for the first time, charged with stealing. It was apparent to everyone that the lad was much afraid. His pupils were dilated and he stammered excessively. Asked his age, he first said 11, then 13, though his mother stated that he was in reality 12, and the boy himself so told us later. When the judge endeavored to speak to him he kept repeating, "Mr. Judge, I didn't do it, I didn't do it," alternating this with, "I am afraid somebody is going to kill me."

As a matter of interest and for scientific purposes, this boy was tested immediately after he left the court room. No effort was made to change his emotional state (but as is always

done he was urged to do his best). He was tested in just the frame of mind in which we found him—his pupils were still dilated but he did not stammer. He was not at all recalcitrant; indeed, he made a decided effort to do what was asked of him. Such tests were tried as could be repeated without the results being improved through practice. It was found that what could be done on the basis of rote memory was done quite well, but where processes of thought or mental representation were required the results were exceedingly poor. The simple crossline test was a failure after four trials; the more difficult one was quite beyond him. Tested for memory span he repeated six numerals correctly, failing on the seven-place numeral test. Asked if he had one dollar and spent 87 cents how much would be left, he repeated over and over, "30 cents."

On Binet tests he graded through 9 years. He failed to give the difference between remembered objects, to define simple as well as abstract terms, to detect incongruities, and to answer common sense problems; he could not form sentences containing 3 given words, nor arrange the 3 words in 2 sentences; though he gave 60 different words in 3 minutes, he repeated some words a number of times; he failed to rearrange the words of the jumbled sentence so that they would make sense.

This boy was seen three days later at the Detention Home and several times after that. Though every effort was made at these interviews to explain that there was no occasion for fear, we felt we were not entirely successful. Often he seemed on the verge of tears, yet he was not nearly so disturbed as when seen in court. The results on many of the tests were decidedly better. Both crossline tests were solved correctly; arithmetic proved difficult for him but he gave the difference between 100 and 87 cents correctly by means of counting on his fingers. By Binet scale he graded one year higher. He now gave the 12-year sentence containing 3 words correctly; rearranged the jumbled sentence; in fact, succeeded on all the previous failures except the incongruities, the common sense judgment test, and the definitions. He gave 40 words in 1′ without any repetitions, and 83 words in 3′, including 3 repetitions.

Case XV.—In another instance, a girl 13 years 4 months was being examined. It was difficult at best to be sure of a diagnosis, because the girl had been a comparatively short time in this country and was handicapped by language difficulties. She was seen by two different examiners on different days. On first testing she graded $9\frac{3}{5}$ years by Binet, but even more significant was the fact that she was unable to do the simple construction test, having failed on it at the end of 5′. She likewise was unable to solve correctly the simple crossline test, though both of these should have been well within her powers, if normal. As a result of the first day's examination it was considered very probable that the girl was distinctly subnormal.

During examination on the second day, she worked interestedly, coöperated well, and seemed thoroughly at ease. The results of the tests, as a whole, were considerably better; not only the simpler construction test but one involving more complex relationships was solved well. The simpler crossline test was correct on the first trial and the more difficult one on the second. One found that she could form new associations promptly; that she had good apperceptive ability and exceedingly good rote memory. Three Binet tests, formerly failures, were now scored as correct, thus giving her a Binet rating of 10 years.

Asked for an explanation of her poor record when first seen, the girl herself said, "I was afraid and nervous the other day." No doubt this was the real explanation of her apparent stupidity and of the decided improvement shown on the second testing.

(*d*) *Sheepishness or Feeling of Shame.*—Sometimes the examinee is not so much afraid or depressed or angry, but is generally ill at ease and somewhat ashamed because of the difficulties in which he has become involved. The mere fact of being examined is, in itself, sometimes a cause of shame. It is not at all uncommon for adolescent boys, particularly, to act in a very sheepish manner.

Case XVI.—A lad of 16, who was first tested before his story was gone into, failed on a number of tests. He could

not do an example in simple long division; he could not succeed with the more difficult crossline test and he filled in the spaces of the pictorial completion test very poorly, although when cross-questioned he could give correctly the meanings, appearing to have really apperceived the situations. All the Binet tests were done well; it was felt that the boy was not doing himself justice on many of the other tests.

Then there followed an interview with another experimenter. After he had unburdened himself and realized that he was not going to be very severely condemned, he appeared much more serious and more at ease. He was asked on the same day to try again a few of the tests previously done, and it was found he now solved the same problem in long division correctly, that he gave the correct solution of the crossline test on the second trial, and did the completion test very well.

(*e*) *Shyness.*—Because of shyness and timidity younger children especially are often exceedingly unresponsive. Sometimes it is impossible because of this to continue the tests and one must first endeavor to make friends. We may note as an example Case XVII, a little boy of 10 years, where our psychological impressions run as follows: "He is extremely quiet and diffident. It is difficult to make friends with him. He seems quite averse to speaking, but it is not due to a lack of knowledge of English. All language tests are failures largely because the boy will not speak."

This first day's testing proved to be practically valueless, for the boy failed on our simple picture puzzle test and on the simpler construction test. On the completion test there were 6 errors, 4 of them illogical. The Binet rating was 8½ years.

No effort was made to retest this little boy for some time. It was thought that after he became more at home and had seen us during his play time, perhaps he would feel less shy when brought to the laboratory. This reasoning proved to be correct, for on the second trial the results were markedly different. The picture puzzle was solved correctly and the first construction test done with a very good record. Not only that, but the more difficult construction test was solved correctly too. The completion test, on retrial, was completed with no errors and his Binet rating was now through 10 years.

(*f*) *Embarrassment due to Onlookers.*—As stated briefly elsewhere, extraordinary conditions, over which one sometimes has no control, may interfere to such an extent that test work is valueless. If visitors are present the examinee may become so embarrassed and confused that he fails utterly on things well within his power. If one wishes to be fair and to learn the best of which the individual is capable, the presence of visitors at examinations should be discountenanced.

Case XVIII.—Recently we had a striking illustration of this point. We were compelled to see a boy when three spectators were present. They were seated in such a way that they faced the boy, who, therefore, never for an instant was unconscious of their presence. His embarrassment was as evident in his general manner as in his failures on tests, which he later did satisfactorily.[1] At the first interview his Binet record was as follows: All 9-year tests correct; three failures in the 10-year group, and three in the 12-year group. He did not solve the more difficult construction test, repeating the same impossibilities over and over in a most stupid fashion. The crossline test seemed easy for him and was correct on the first trial. But on the pictorial completion test he made 2 errors; he likewise made several errors in addition and became hopelessly involved in a problem in long division.

Several days later this boy, 16 years of age, was seen alone. Now all the 10- and 12-year Binet tests were correct except definition of abstractions in the 12-year group; no errors were made on the completion test; the construction test was done in 44″ with no incorrect or unnecessary moves. Number work was still poorly done, this being the field of his greatest weakness. The boy himself said, "I got rattled the other day, there were too many people around."

[1] Of course, if visitors make any kind of comment the situation is all the worse. This is illustrated in the case of a girl first seen in another clinic where visitors were present. With us this girl did test work very well, indeed, showing a very nice attitude. At the completion of the work she told us in detail of her previous experience, and that she had not only been greatly embarrassed, but much humiliated because of the obvious recognition of her failure by these visitors. She acknowledged that after noticing their amused smile she felt resentful and angry and made no further effort to succeed. Our positive findings rank this girl as quite fair in ability, whereas the verdict of the first examination had been that she was a mental defective.

(*g*) *Homesickness.*—The emotional disturbance, due to homesickness, is, in reality, a kind of mental conflict. This has been made much of in foreign literature.

Case XIX.—An instance in which it is a factor is next presented: Seen and tested on several occasions the most significant feature, in general, regarding this case is that the results varied greatly from time to time. There was no explanation of this other than the emotional conditions under which we found the boy on different days. He cried very readily, told us of his extreme homesickness, and when one day his mother came to visit him and the two met in the laboratory we ourselves saw a long-continued display of emotion on the part of the boy.

It was soon noticed that he was lacking in powers of concentration and attention, but that he could put his mind on what was asked of him when he was interested, and that his attitude was largely dependent on the way one approached him. The first day this little boy was seen he was most unhappy and homesick; he cried bitterly. He failed on many of the tests, even the construction tests, which later interested him greatly. His Binet grade was $8\frac{3}{5}$ years. He read poorly even a first-grade passage and failed to do a simple problem in addition.

Seen several days later he showed considerable improvement on some of the tests first tried, then once more was overcome by his emotions, cried again, and test work had to be stopped. It was on this second occasion that his mother came while he was working.

At the third interview Binet tests were given again, with the result that he then graded $9\frac{4}{5}$ years. The construction tests, previously failures, were now solved correctly. He succeeded with the simpler crossline test on the second trial, changed his record on the completion test from time reaction of 7′ 17″ to 5′ 12″, 5 illogical errors to 2 logical ones. Thus the results as a whole were quite different from the first ones.

(*h*) *Mental Conflict.*—Before leaving the general subject of emotional disturbance we must discuss, if only briefly, the problem of mental conflict. Naturally conflicts, on whatever

basis they arise, cause great emotional upheavals, and these we would expect to find reflected more or less vividly even in the reactions to tests. This is not true in every case. The victim of some conflict may show emotional distress only when his particular problem is approached, or when some association is evoked which is connected with it. But there are instances where worry or unhappiness on the basis of some conflict is so great that it influences all reactions. Almost any type of behavior may be the result.

Case XX.—This is well illustrated in the case of a boy 12 years old when first seen. Everyone who came in contact with him felt that there was something unusual in his mental attitude; school teachers, attendants in the home where he was being held, even his mother commented upon this fact. He was seen in the laboratory a number of times. There too it was felt that the boy displayed extreme obstinacy and recalcitrancy. He would work well on a few tests, then would sham disability, and at times refuse absolutely to coöperate. At the beginning of testing he was decidedly surly, saying he did not like any games. He never smiled, nor in discussing his own delinquencies did he show any remorse or approach tears. One was sure that he was not making his best efforts. As in an earlier case (Case X) this boy did better on performance tests. He solved the simpler construction test in 2′ 5″, making in all 15 unnecessary moves. At the expiration of 2′ he himself said, "I can't do it," although with 5″ more effort he succeeded. He failed on the simpler crossline after four trials, but succeeded on the more difficult one on the first trial.

Seen a month later the boy not only did the simpler construction test in 30″ (not a very significant performance, since he may have remembered the solution) but the more difficult construction test was done in 35″ with not a single unnecessary move. The simpler crossline test was correct on the first trial. This boy has been seen on numerous other occasions at varying intervals, and we have come to the conclusion that he is quite capable, and that his earlier failure was not due to lack of ability.

It was felt from the start that in this case the mental

attitude was not that of defiance, nor did the boy display a grudge-like attitude. It was simply a matter of stubbornness and recalcitrancy. After repeated interviews it was definitely proven that this attitude was based upon a conflict regarding sex affairs, which colored this boy's reactions toward everyone. Of course in a case of this sort the appreciation, not only of the attitude itself but of what causes it, is of great importance in relation to social behavior as well as to the interpretation of test results.

V. *General Nervous Excitement.*—The very effort to do well may sometimes prove as disastrous as its direct opposite, namely, indifference or ennui. We have seen instances where the examinee in his effort to attain an exceedingly good result, becomes so excited that he defeats his own ends.

Case XXI.—After seeing a girl 17 years of age it was stated; "Test work with this girl was very unsatisfactory, not because the girl would not coöperate, but because during the work she became a good deal confused. This was due to the very effort she made to do well. One would hesitate to judge of her capacity by the results of this interview, for it was evident that the girl was not doing herself justice; indeed this was so apparent that it was hardly worth while to do more with her."

Fortunately we were able to study this case further about two weeks after our first interview. This time she was quieter and more controlled. A comparison of the results on the two occasions shows significantly the truth of our earlier impressions. Though a grammar-school graduate, on the first examination she succeeded on the crossline test only with great difficulty; on the code test she made 7 errors besides omitting all the dots; she made a very poor record on the Kraepelin continuous subtraction test and 3 errors on the antonyms test. At the second interview the crossline, with letters substituted for numbers, was done promptly on first trial; the code with 2 errors but no dot omitted; the antonym test still had 3 errors, but the average time was somewhat better.

Case XXII.—A still better example is the case of an exceedingly bright boy not yet 13 years old, who showed great eager-

ness to make an exceptionally good record. He asked continually if he were doing better than his older brother, and grew excited at his own successes. He showed this by his poor mental and psychomotor control as well as by his general reactions. No effort was made to stimulate him, but on the other hand no attempt was made to quiet him at the first interview. The boy himself expressed a desire to come again to the laboratory and try again 'the games,' as he called them. At the second interview he was urged to work quietly, to take more time, and to be more deliberate; he was reminded of these instructions once or twice during the work. He himself saw the advantage of this and made an effort to control himself. It is interesting to compare the results of the two days' work.

The construction tests are not very illuminating, because the boy remembered the solution and so naturally made a better record. But the simpler crossline test, correct only on the fourth trial the first day, was now done easily on the first trial, though the letters were used in place of numbers and the order of placing the letters was altogether changed. The second crossline, a failure the first day, was promptly solved on the first trial, with change of numbering and placing of the numbers. The record on the tapping test changed from 87 dots with 6 errors and 78 dots with 2 errors, to 76 dots 0 errors and 77 dots 0 errors.

On the easy opposites he made 1 error at each of the two test periods, but the average time of reaction was 2.3″ in one instance, and 1.5″ in the other. The range is 1.2″ to 5.8″, as compared with 1.2″ to 2.4″. Subtracting continuously 6 from 75, his record changed from 8 errors to 2 errors. Furthermore, the first day's work here showed great lack of control, the following being the typical responses: 79, 63, 57, 61, 55, 89; time required, first trial, was 2′ 18″ and 1′ 46″ on the second trial.

VI. *Lack of Confidence.*—Sometimes the attitude is more nearly that of cowardice than anything else. The boy or girl falls back upon the statement "I can't do that," and left unencouraged will remain content to fail. It is closely akin

to the discouragement that Ruger and Swift found in their adult subjects. Such persons are not so much incapable as averse to making an effort.

This was true in Case XXIII, a boy 13 years 6 months, brought to us as "probably feeble-minded and needing to be sent to the state institution." We could not agree with this diagnosis made, it is true, not on the basis of tests. We found that the boy was a school failure and that he believed himself incapable of doing anything intellectual. Urged to try, he did a construction test correctly, but he wished to give up altogether when the simple crossline was a failure at the second trial. He was made to continue, and he succeeded not only on this test but on the more difficult one. The same trait was shown throughout his work. Thus he succeeded in giving sixty words in less than 3′, though he said repeatedly, "That is all I know." Such cases require patience on the part of the examiner and frequent use of praise and encouragement.

VII. *Combination of Causes.*—In some cases the explanation is not so simple; the problem may not be an easy one to analyze. Sometimes it is evident that there are disturbing conditions, that the mental attitude is unsatisfactory, but one does not feel sure just wherein the trouble lies. Two or more conditions may seem to be factors, and this makes it at times exceedingly difficult to reach any definite diagnosis.

Obvious recalcitrancy may be upon the basis of some conflict or grudge, in this respect differing from the case cited above, where no motive was ever discovered; or, bad physical conditions may be present and it may be difficult to know just how much allowance should be made for this. We cite one instance where a number of factors complicated the situation to such an extent that only a tentative diagnosis was vouchsafed, even after repeated study. Not until several years had elapsed and some of these factors were eliminated, was enough confidence felt in test results to make us willing to offer a definite statement regarding this girl's mentality.

Case XXIV.—A little girl, 10 years 3 months, was studied after she had already been very delinquent. When first seen,

she was exceedingly downcast; she wept copiously and frequently. In the school-room she was said to be naughty and stubborn. It was known too that she was engaging in bad sex habits.

The most striking feature of early work with this girl was the excessive inhibitions shown on tests, in conversation and in the school-room. After a few tests were done she would put her hands over her face, avert her eyes, refuse to speak, and begin to weep bitterly. Although seen on three different occasions, on all of which the greatest kindness was shown her, the diagnosis of her mentality was left an open question. At the first interview she made a fairly good record on the simple picture puzzle and on the simpler construction test. She failed to solve the more difficult one, refusing to work at the end of 7′. Seen the next day she failed on the simpler crossline test after four trials, refused to attempt the more difficult crossline test, would not answer any tests where the solution required the use of language, refused to do any school work.

It can readily be seen that at this time one could not possibly state whether this girl was unable to do the tests, whether she merely refused on a basis of recalcitrancy, whether she was dull from her bad sex habits, whether she was developing a psychosis, or whether her peculiar inhibitions were due to some mental conflict which her experiences, too long to recite here, made possible.

Seen a year later, in general her reactions were much the same. She still showed interest in performance tests where she could handle concrete material, but the tests previously failures were still done as poorly. The girl refused to attempt Binet tests involving language, and in more difficult tests where continued effort was required she refused to coöperate any length of time.

Just recently, more than four years after first knowing the girl, we have once more studied this peculiar problem. We find her attitude much changed. She has been for a long time in an institution where a great deal has been done for her. At our last interview she coöperated apparently as far as she was able, was friendly, and showed none of the extreme inhibi-

tions previously so characteristic. We feel now that the results of tests are trustworthy and give us a basis for diagnosing her mentality. We find that she still does performance tests extremely well; she succeeds with the simpler crossline test, but is still unable to cope with the more difficult one. By Binet tests she grades through all of the 10-year series and does well three of the 12-year series. She has had good school opportunities, but is quite retarded in this respect. In number work she can only do problems in addition, while her reading for a girl of her age is distinctly poor. From these results, we must conclude that the girl is subnormal.

The final diagnosis in this case is not the point of greatest interest from the standpoint of our present discussion. It illustrates well the time and care that are sometimes needed in the understanding of a problem. There is no doubt that when first seen all the significant features noted were real factors. There are many cases where the only fair conclusion is that no definite decision can be reached. Only follow-up and retesting can lead to a diagnosis that is at all reliable and worth while.

Turning to the positive aspects, we must emphasize particularly several factors. Not only should thought be given as to the time and place of examination, and not only must the experimenter possess sympathy, tact, and skill, but he must win the subject's active coöperation. We would stress the value of presenting an aim or motive that has meaning for the individual. The experimenter must beware of simulation on the part of the examinee, and he must analyze sufficiently all reactions, particularly negative findings, in order to know whether they are really indicative of disability, or whether they are symptoms, or even possible symptoms of disturbed conditions.

The most favorable attitude, no doubt, is that of the ambitious, but well-controlled person, the person who does not allow his emotions to interfere with his mental processes, and who yet is interested enough to exert himself to his best efforts. The particular means of creating this attitude varies with each individual, but we can only hope to achieve or approach it by recognizing the various pitfalls and by endeavoring to avoid them.

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THE NATURE AND PROBABLE ORIGIN OF BINAURAL BEATS

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If two tone waves of slightly different pitch are conducted separately in tubes, one to each ear, it will be found that they beat. Beats of this kind are called binaural to distinguish them from beats produced by the operation of both tone waves in the same ear. A question of importance to auditory theory arises here: Are these two kinds of beats, binaural and monaural, really different in principle? Are binaural beats not due to some sort of conduction of the vibration series from one ear to the other so that both waves really operate in each ear? At least three modes of such conduction from one side to the ear at the opposite side of the head are possible. Unless much care is taken in connecting the sound-tight tube to the meatus of the ear, vibrations may be communicated to the air external to the head and thus carried to the opposite ear as an ordinary air wave. As a second possibility the tone wave may go directly from the air in the meatus to the bones of the skull and thence to the liquids of the inner ear on the opposite side of the head. Finally, some of the middle ear structures, or even the liquids of the labyrinth, may communicate the wave to the bones of the skull by which they can be conducted to the opposite ear. It is conceivable, moreover, that under certain conditions all these means may be operative.

Evidence is not wanting to show that such cross-conduction of the wave is probable. It is well known that vibrations are effectively conducted to the labyrinth by the bones of

the head. This is illustrated by the Weber test. If the stem of a sounding fork is held against the skull at the right ear, or at any other place, while the meatus of the left ear is plugged with the finger tip, the tone will be clearly perceptible in the left ear even though the vibration of the fork is very weak. Auscultation of the skull roof by means of a sensitive microphone has shown that the bone may take up the wave.[1] Wilson and Myers report[2] that by leading the tone of a tuning fork to one ear of an individual they 'proved that an appreciable amount of sound could be transmitted through his head to the other ear and could thence be conducted by a tube so as to be audible to the ear of a second observer.' It is well known that even inaudible interfering tones may produce clearly perceptible beats. Therefore it is not safe uncritically to assume that faint tones are not transmitted to the opposite ear. S. P. Thompson found, for example, that two Ut_3 ($c' = 256$ v.d.) forks, slightly mistuned, produced very distinct beats when their tones were conducted in tubes one to each ear even though the tones were so faint as to be 'all but inaudible' when sounding individually.[3] For these reasons, and others to be considered later, there is a tendency on the part of certain investigators to attribute all binaural beats to interference of vibrations in the same labyrinth, due to bone conduction.

Certain experiments have produced evidence of a peculiarly forceful kind against this interpretation of binaural beats. Cross and Goodwin[4] in a carefully conducted investigation attempted to exclude cross-conduction of waves. That they were entirely successful in this attempt is not here maintained. Rods connected with the stems of the sounding forks were held against wax plugs in the ears. Binaural beats were plainly audible even when the tones were very weak, but difference tones could not be obtained, with the necessary pitch difference of the forks, though the intensities were greatly increased. If, however, one of the rods was

[1] Nagel, 'Physiologie des Menschen,' 1905, 3, 575. References are there given.
[2] *Brit. Jour. Psychol.*, 1906, 2, 381.
[3] *Phil. Mag.*, 1877, (5) 4, 274–276.
[4] *Proceedings of the Acad. of Arts and Sciences*, 1891, 27, 1.

held against the skull or the teeth while the other was in contact with the wax in one ear "a loud differential tone was heard at once in the ear against which the rod was placed." In such a case both the vibration series could operate together in the one ear by means of bone conduction from the rod touching the skull. The intensity of the beats did not seem to be affected by this change, however, as we should expect if the beats were also due to the action of both waves in each ear. This experiment certainly suggests that bone conduction was not very strong, if indeed it was possible at all, until one rod was held against the skull. Difference tones are possible only when both tone waves operate in the same medium either at the source of the tones or in the ear. This general statement of the conditions under which combination tones are possible is, I think, agreed upon by all authorities irrespective of bias as to any particular theory of the origin of such tones. This is one of the strongest pieces of evidence against the view that binaural beats are to be explained on the basis of bone conduction, yet no one maintaining this view has considered seriously, or at all, it seems, the results of the experiment here mentioned.

Binaural beats have been studied incidentally of late by a number of experimenters who were primarily concerned with the perception of phase differences of waves at the two ears. These investigations have been carried on mostly by physicists under conditions so carefully controlled that there can be little room for differences of opinion as to the general results.[1] It is now unquestionably established that phase differences are perceived, but the explanation of the whole matter is still in controversy. Wilson and Myers maintain that such perception is after all based upon intensity differences at the two ears, and they have explained in an ingenious manner how all phase differences at the ears may conceivably be converted into intensity differences with the consequence that the greater intensity occurs at the ear receiving the

[1] Rostosky, *Phil. Stud.*, 1902, **19**, 557. Lord Rayleigh, *Phil. Mag.*, 1907, **13**, 214, 316. More and Fry, *Ibid.*, 1907, **13**, 452. Bowlker, *Ibid.*, 1908, **15**, 318. Wilson and Myers, *Brit. Jour. Psychol.*, 1908, **2**, 363. More, *Phil. Mag.*, 1909, **18**, 308. Stewart, *Physical Rev.*, 1914, **3**, 146; **4**, 252. Schulze, *Annal. d. Physik*, 1914, **45**, 283.

earlier phase. Their objection to the view that phase differences are directly perceived, as Lord Rayleigh, More, and others have assumed, is that it compels us to depart radically from our present conception of the origin and nature of the nervous impulse. "We have hitherto believed that, under ordinary circumstances, all sensory stimuli (save, possibly, painful ones) act on the nerve fibres solely through the intermediary of end organs; and that these organs may in certain cases be excited by various kinds of stimuli—mechanical, electrical, chemical. Since these diverse kinds of stimuli give rise to similar sensations, 'we have hitherto believed that the impulses ascending a sensory nerve depend on the mode of response of the end organ and not directly on the character of the stimulus.'[1] "It is very hard to believe that every crest and every trough of each sound wave produce an exactly corresponding crest and trough in the impulses transmitted along each auditory nerve."[2]

The theory of Wilson and Myers is based on three suppositions: (1) That the sound wave is transmitted to the opposite ear by bone conduction; (2) that the retardation of phase due to this ear-to-ear transmission is small; (3) that the two waves, the direct and the transmitted, arrive in each of the cochleæ from opposite directions. These suppositions, which are crucial to the theory, have been questioned, under certain conditions, at least, by More,[3] who has repeated the experiment of Wilson and Myers. More also tried higher-pitched forks than any of those used by these experimenters—128, 180, 240, 256, 320, 384, and 512 v.d. He found that 512 v.d. was near the limit of accurate lateral location by phase differences. Above this the accuracy decreases, becoming untrustworthy at about 1,024 v.d. No systematic tests for continuous pitch differences above 1,024 v.d. were made, "yet a qualitative trial [under the accurate conditions given by the use of the Wilson-Myers apparatus, however] with a fork of pitch 3,000 v.d. approxi-

[1] *Op. cit.*, p. 377.

[2] *Ibid.*, p. 378.

[3] More, L. T., 'On the Localization of the Direction of Sounds,' *Phil. Mag.*, 1909, 18 (6th Series), 308–319.

mate, showed that absolutely no sensation of direction existed."[1] More reports, moreover, that tones "so feeble that sufficient sound did not travel through the head to produce any sensation" were as easily localized by phase differences as were loud tones.[2] Suddenly pinching one of the tubes, thus instantaneously decreasing the intensity at one ear, did not, as would be expected on the Wilson-Myers theory, cause a confusion in the judgment. Extreme changes do, of course, cause a confusion, but very slight ones should do so if the perception of phase difference is reducible to perception of intensity difference. More also points out the obvious fact that according to the Wilson-Myers theory tones should be more accurately located than noises, "while the contrary is proved by experiment." With a subject completely deaf in the left ear More found that a tone, even though intensified by a resonator, could not be heard in the right (good) ear when conducted to the left one. This is strong evidence against the cross-conduction of a perceptible amount of vibration through the head.

My own experiments in acoustics, at first repeating the experiments of various investigators, early led me to doubt the explanation of binaural beats, and of the perception of phase differences, based upon cross-conduction of the wave through the bone. That such conduction is not only probable but actual under certain conditions, no one can deny. Repeated tests which I have made chiefly with forks c′ (256 v.d.) and a′ (435 v.d.) show that the perceptibility of vibrating forks held against the skull on different places varies considerably with equal intensity of vibration. A fork, *e. g.*, which becomes inaudible on almost any other part of the skull may again be heard if held against the mastoid process behind the ear. The sensitivity to a fork at this point is almost equal to that of one held against the teeth, yet when the fork has 'run out' at either of these two positions it is easily perceptible when held with the prongs just before the ear as closely as possible without making actual contact.

[1] *Op. cit.*, 314.
[2] *Ibid.*, 318.

If now, when the fork is held against the mastoid process, a finger tip is inserted into the opposite ear it is found that the tone is not only located in the stopped ear, but that it is perceptible in this ear with fainter intensity than in the unstopped ear nearer the source of the sound. This high degree of sensitivity of the opposite (stopped) ear has been used as evidence that binaural beats are really due to bone conduction; *but such a view overlooks the important fact that the tone waves so easily perceived in the plugged ear do not come to the cochlea directly from the stimulus through the skull, but are reflected back from the finger tip and enter the ear through the usual channel.* Wilson and Myers found that if a tone is conducted to the subject's right ear, for example, a person at the opposite side might hear the tone through a tube connecting his own with the subject's left ear. This experiment obviously does not prove, as it has been thought to prove, that cross-conduction waves are easily perceptible in the opposite ear when not stopped or plugged, *i. e.*, under the conditions of the Wilson-Myers experiment proving the perception of phase differences.

Binaural beats, of the peculiar quality soon to be noted, may be heard with both ears unstopped, or normal, even if the feebly sounding forks are not even in contact with the head, but are held before the ears. The fact is, therefore, that the ear is insensitive to a high degree to tone waves coming directly from the skull, and highly sensitive, on the other hand, to those which come through the usual channel from the meatus. In the case of the plugged ear the waves from the skull are perceived, for low intensities, only when reflected back from the direction of the tympanum by the finger tip.[1] *This fact is in direct opposition to the third of the Wilson-Myers suppositions*, which can therefore be valid only in a small degree and then merely for intense vibrations.

It was the nature of binaural beats that led me to discredit the cross-conduction theory of their origin, much as the theory was consoling to my own view in a time of per-

[1] Cf. Mach, *Berichte d. Wiener Akad., math.-nat. Kl.*, 1863, 48 (2), 283, cited by Schaefer, K. L., in Nagel, *op. cit.*, 574.

plexity. On the nature of binaural beats I have gathered—as a control test to my own observations—unbiased descriptions from a number of competent observers. Forks sounding with as nearly equal intensity as possible were held in irregular, or chance, order either one at each ear or both at the same ear. The forks—c′ in this case—differed slightly in pitch so as to beat very slowly, approximately once per second, and the observers were asked to write out descriptions of the beats. They were at the time ignorant of the use to be made of the report and were not informed on this matter until quantitative tests had been made, following the descriptive reports, to determine the accuracy of the counting of binaural beats. The results here given are only those obtained from experienced observers, instructors in psychology, and are in substantial agreement with those obtained from less experienced persons. The observers will be designated as *Kr*, *D*, *Kg*, *W*, *E*, and *J*. All did not take part in every test.

Kr describes binaural beats as constituting a 'unified experience with fluctuations from side to side.' "Monaural beats are discrete experiences. There is a series of wide gaps which seem to break up the total sense experiences. These gaps are of varying size," with different relative and absolute intensities of the primary tones. There is no uctuation in the location of the beats.

E says: "Monaural beats are much more distinct and more easily counted, definitely localized, and do not shift as do the binaural beats." In counting there was 'less effort of attention required and no doubt, but rather a feeling of certainty' as to the accuracy of results. "Binaural beats less distinct, feeli g of effort clearly present in my trying to be sure of the count; slightly unpleasant; localization not as definite as in case of monaural beats; beats are often localized at one side or the other, but also very often at a fairly large region around and over the head. The sound seemed to fill this region, about two feet in diameter; as the phase changed the sound seemed to run to the opposite side across upper back part of the head, occasionally fairly vivid visual

images of narrow bands of light going back and forth in this way."

D, whose right ear is partially deaf, had extreme difficulty in counting binaural beats, and was therefore excused from the beat counting tests. The sound was continuous at the good ear, and while there was no shifting of the sound he experienced slight beating effects which he counted as slower (in the ratio of about 2 to 3) than the beats which actually occurred. For this observer the fork at the right was held nearer than the one at the left ear.

The results of *Kr* and *E* are typical and agree well with those reported by other investigators. For example, Cross and Goodwin report that two forks were held against the teeth until their beats (4 per second) were no longer audible when suddenly they were removed "and the stem of each was touched to the wax closing the two ears. Instantly the two notes were heard, faintly but distinctly, in the ears to which they were held, and accompanying them were faint beats seeming to wander in the head from ear to ear, as is always the case with binaural beats."[1] The beats in this case were counted correctly, as is usual with frequent binaural beats. As the beating rate increases the shifting from side to side, becoming too rapid to follow, gets to seem more and more like true interruptions, as with monaural beats. It is also evident that if there were here a small amount of cross-conduction, contrary to the opinion of the experimenters, the crossed waves would not likely produce directly a perceptible effect in the cochlea, but would be reflected back to the cochlea from the meatus along the usual channel of auditory sound waves. In such a case the beats would be monaural in each ear and would be easy to count.

For some time my experience has shown that binaural beats, within certain limits, become easier to count as their frequency increases. Very slow beats, especially of low pitched forks, are readily perceived to be located now on one side and now on the other, and to shift with the change of phase. As they increase in frequency the effect is to pro-

[1] *Op. cit.*

duce something of a circular motion very hard accurately to count, and at a still greater frequency this shifting approaches more nearly the nature of the interruptions of monaural beats, with no changes in location. The point of greatest error in binaural beat counting seems from my own observation to be at a frequency of about one beat per second, at which point the swing to each side is often counted by the unsophisticated as a separate beat, thus giving the result of multiplying the actual beats by two. In my first experiments precautions were taken to have the tones separately conducted to the two ears in tubes. The end of each tube was wrapped with a handkerchief and made to fit into the ear in such a way that the air wave could presumably not escape and enter the opposite ear by passing around the head. Tone waves were also separately conducted to the ears by means of metal rods or heavy wires each held in contact with a particle of wax in the meatus of the ear to which it led. The wax thus forms a resonating cavity in the meatus. The stems of the vibrating forks were held against the farther end of the rods. With moderate intensities the results under these conditions were precisely like those obtained by holding unresonated forks directly to the ears. Consequently the more elaborate methods were abandoned for the simpler one so that such conditions as the relative intensity of the tones could be better controlled. Another objection to the wax-in-the-ear method is that the wax stopper will reflect back the cross-conducted wave—if there be any—and make both waves enter each cochlea from the same direction, whereas we are endeavoring to find out what takes place under normal conditions when there is no such reflecting back of the crossed waves. Below[1] will be found results obtained with ears stopped as compared with ears normal, the stems of the forks being held against the skull in each case. These results amply justify the abandonment of the wax-in-the-ear method, valuable as it proves to be in certain studies.

The following results were obtained with two unresonated c′ forks of slightly different pitch. These were held in

[1] See pages 345, 346.

chance order a short distance before the observer's ears either both to the same or one to each ear. Precautions were taken both in actuating the forks and in the manner in which they were held to the ears to insure a practical uniformity of experimental conditions. The experimenter sat directly behind the observer, and followed a chance order of experiments both as to the frequency of beats and as to the method of stimulation just described. The order was the same for all observers. Because of the defectiveness of *D*'s right ear he was not used in this beat-counting experiment. *W*'s right ear was slightly defective, so the fork at this side was always held closer than that at the left. The observer watching the second hand of a watch began counting the beats at a favorable point and continued for ten seconds. *W* used a stop-watch, and began each time by the more accurate method of 0, 1, 2, etc. The other observers began counting with 1. No deductions for this were made as the results are wholly for comparison between monaural and binaural beats. The beats recorded are the total number counted during the ten seconds.

The pitch of one of the forks was changed, as the program required, by the shifting of a small brass clamp which was attached to one of the prongs. Three marks were made on the prong so that the number of beats for each position of the clamp would be as nearly constant as possible. The slight variations in the counts of each observer of monaural beats are unquestionably to be accounted for in part by the impossibility of putting the clamp at exactly the same point each time for a certain frequency. Even when the chance order required no change in the position of the clamp a change away from and back to the mark was made so that "O" would remain ignorant as to the number of beats to expect. Occasionally it was desirable to set the clamp in an unusual position, and get an unusual frequency of beats, so that "O" would not learn the three particular frequencies to expect. This was usually unnecessary, however, as the results for binaural beats were so variable.

In the tables which follow *N* represents the rapid fre-

quency, about 24 in the ten seconds; *n*, about 11.5 in the same period; and *n′*, about 3.25. Question marks indicate a high degree of uncertainty on the observer's part. In the cases indicated by footnotes unsuccessful attempts to count the beats were made. Usually these were in immediate succession; occasionally some cases of monaural beat counting or short periods of rest were interspersed. Other features of the results are self evident. A little practice was given each observer before results were tabulated.

Observer *E*						Observer *Kr*					
Binaural			Monaural			Binaural			Monaural		
N	*n*	*n′*	*N*	*n*	*n′*	*N*	*n*	*n′*	*N*	*n*	*n′*
21	10 ?	6 ?	24	12	4	24	24	30*s*?	25	13	4
21	10	6 ?	24	12	4	26	20	22	24	13	4
22	12	6	24	12	3	27	20	30 ?	23	12	3 ?
12*s*[1]	8 ?	6	24	12	4	24	28	25 ?	26	12	4
16	12	8 ?	24	12	4	27	24	23 ?	24	12	3
20	6 ?	6 ?	24	12	4	19	11 ?	19 ?	27	12	4
9	9	4	24	12	4	38	17	13	25	12	5
24	10*f*	4	24	12	4	25	11 ?	28*f*?	25	12	3
20	9	4	24	12	4	29	20 ?	14	25	12	4
22	10	4	24	12	3	25	20	10 ?	25	12	4
M.... 18.7	9.6	5.4	24	12	3.8	26.4	19.5	21.4	24.9	12.2	3.8
M.V.. 3.8	1.3	1.1	0	0	0.3	3.2	3.9	5.9	0.7	0.3	0.6

Observer *W*						Observer *Kg*					
Binaural			Monaural			Binaural			Monaural		
N	*n*	*n′*	*N*	*n*	*n′*	*N*	*n*	*n′*	*N*	*n*	*n′*
20	5	?*t*	25	12	4	8	5	8	25	13	4
22	4	2*s*?	25	11	4	6	3	7	25	12	3
24	6*s*?	2	24	12	3	12	9	10	23	12	4
24	9	2 ?	25	12	3	7	6	8	25	12	4
25	2	1	25	12	3	22	10	5	24	11	3
25*f*?	4	1*t*?	25	12	4	7 ?	3 ?	4	24	12	3
22*t*?	?*t*	1*t*?	25	12	3	4 ?	8	4	24	12	4
25	2 ?	?*t*	25	12	3	23	8	3	24	12	4
26	?*t*	?*t*	25	12	3	16	3 ?	3*s*	24	11	4
22	?*s*	2+*t*	25	12	3	11*s*?	7	5 ?	25	12	3
M.... 23.5			24.9	11.9	3.3	12.6	6.2	5.7	24.3	11.9	3.6
M.V.. 1.6			0.2	0.2	0.4	5.6	2.2	2.0	0.6	0.4	0.5

[1] *s* = second attempt (the first being unsuccessful); *t* = third attempt; *f* = fourth attempt; ?*t* = failure after third attempt. Note that averages and deviations are of little value in the binaural beats because of uncertainties, failures, and in some cases marked improvement on the slowest beats, the latter due, likely, to some sort of association with the monaural slow frequencies. Fractions are omitted in the observers' reports. When fractions were given the next higher whole number was recorded if the fraction was as large as .5.

W's defective right ear seems to have made his perception of binaural beats very uncertain. In the cases of failure *W* declared that he heard beats but that they were not sufficiently distinct to be counted. It is probable that, even though the fork at the defective ear was held nearer than was that at the good ear, the relative intensities for *W*'s hearing were after all not the most favorable. No complaint of this was made however. *Kg* mentioned several times, when he gave small numbers for the frequent binaural beats, that he heard also more rapid beats. *E. g.*, when 16 was given: "I heard also a slower and a faster beating; don't know whether I gave the right one." As a rule the observers expressed much lack of certainty as to their results on binaural beats in general; question marks indicate only the places of greatest doubt.

Some peculiar individual differences are noticeable in the results. *Kr* shows a tendency to be influenced considerably in the more uncertain judgments by the easily perceptible beats in the rapid series. Frequently he reported for the slow beats: "Too fast to count." This tendency was so strong that in the practice series, which by chance began with the most frequent binaural beats, he 'counted' the slowest monaural beats as 22, 12, and 28. He declared that they were 'practically continuous.' It was found necessary to warn him that some of the beats were very slow. The next count on the slow monaural beats was still wrong, 28 instead of 3, but when the second one came round he gave a triumphant smile, and made no errors after that time. But this discovery of slow beats seems to have had no effect on the binaural slow series. It is interesting that the experience with difficult frequent binaural beats so toned up *Kr*'s expectation that his attention was wholly diverted from the very slowly changing tone bearing the monaural beats of approximately .4 per second.

The tendency which my earlier experiences had led me to expect, to count the swing to each side of the one-beat-per-second monaural series as a separate beat, is evident in the results of only one observer, *Kr*, and even these results

may be due to association with the more rapid beats which happened, unfortunately, to have just twice the frequency of the middle series. Of course the 'influence' may also have been in the oppos te direction. At the frequency of approximately one per second, binaural beats seem to be a continuous sound wandering about in an ell pse, and one can subjectively ascribe one of several rhythms to them, as to the ticks of a clock. Extremely cautious observers are therefore likely to let many 'beats' go by uncounted, and to have grave doubts as to the accuracy of any of their results. This continuous nature of the sound, even for weak tones, in the case of slow binaural beats speaks strongly against an even indirectly appreciable amount of cross-conduction from ear to ear. The results of the trained observers do, however, show, as was expected from the earlier experiments, that of the three frequencies used the beats of approximately one per second are the most difficult accurately to count. It is at this frequency that inexperienced observers seem to be most likely to count each side shift of the tone as a separate beat, thus making the error of multiplying the actual number of beats by two.

As a rule beats are easily counted when the stems of the forks are held against the skull, if the vibration is intense or even of medium intensity. If the forks are pressed hard against the skull the beats become more distinct and approach the nature of monaural beats. The shifting of the sound from side to side is more noticeable when the forks are held one at each side of the cranium than when they approach the median plane. If the intensity is low and the forks are held against the skull near the ears, one at each side, the counting of the beats becomes very difficult and uncertain even though both tones are clearly audible. A test on Mr. *E* shows counting under these conditions to be almost entirely guesswork. The results are more variable than his counts of binaural beats given above; *e. g.*, beats at a frequency of 1.2 per second were counted in successive trials (other frequencies being interspersed, as explained in the foregoing, to keep the observer in ignorance of the frequency used) as

follows: 4.5, ?, 12, 9, 3. The stems of the forks were held against the mastoid processes behind the ears. Here again the rapid beats were more accurately counted. The full table of results is not given because it was extremely difficult to control properly and keep constant both the intensity of each fork and the degree of pressure on the skull. Even when the experimenter could hear the beats plainly with the forks 45 to 60 cm. away the observer was unable to count them correctly. I was surprised several times when the beats seemed intense that the observer did not get them by air conduction around the head. On the contrary he pronounced the experiment 'harder than any I have had.' This experiment shows how much the possibility of air conduction around the head has been exaggerated by some writers. Caution, however, where there is uncertainty is a good thing.

This experiment was repeated with the change, that the observer was asked to put a finger tip into each ear. Under this condition *E*'s results with ten trials on each of the three frequencies of beats shown in the tables above were precisely of the same degree of accuracy as was his counting of monaural beats. In thirty-five trials not an error was made. *E* described the beats as very distinct and clear. It was evident that we now were getting monaural beats in each ear. The location of the sound did not shift, as it does with true binaural beats. Since there is no reason for the existence of more bone conduction with the ears stopped than with them open, it appears *that vibrations may be sent across the skull from ear to ear with a considerable degree of intensity and yet not produce beats in the opposite unstopped ear with another tone.* The auscultation experiments, which have been used to prove the effectiveness of bone conduction in binaural beats, and in the location of sound by phase differences at the two ears, therefore prove nothing in this regard. On the contrary, it is highly probable from the results here given that the effect of such cross-conduction is entirely negligible.

Convincing as some of these results from normal observers may seem to be against any explanation of binaural beats

that assumes bone conduction from ear to ear, it is still desirable, if we are to avoid any possible error in our conclusion, that a study of bone conduction be made on a monaural observer. More's experiments were carried out in part on such a person, and confirm our results perfectly. "Miss *S*., after an attack of spinal meningitis at the age of four years, entirely lost the sense of hearing in the *left* ear. Examination by aurists showed that the nerve of this ear was atrophied, and the power of hearing gone." The good ear, we are told, 'was abnormally acute.' More found that when the right (good) ear and the tube leading to it were stopped with cotton no sound was heard. The result was not different even when the fork, whose tone was conducted to the ears, was intensified with a resonator. That is to say, a loud tone conducted only to the deaf ear was not audible in the good ear by bone conduction even though the latter was plugged with cotton. Beats may, however, be audible even though the primary tones interfering cannot be heard. Could *S* have perceived beats if moderate tones of slightly different pitch were conducted separately one to each ear? No such tests seem to have been made.[1]

Before seeing the report of More's experiment I had planned independently more extensive tests on a monaural subject. The experiment was carried out somewhat over a year ago in the psychological laboratory of the University of Chicago. An observer, *J*, was found who is entirely deaf in the left ear both for high and for low tones. This condition resulted from an attack of scarlet fever in childhood. The right ear is apparently even better than the average in its acuity for tones. An unresonated fork of 768 v.d. was used. With a certain intensity (determined by the impact actuating the fork) which made the tone just audible at a distance of approximately 60 cm. from the good ear, no sound was perceptible at all when the fork was held at the left side of the head. With greater intensity of vibration the fork on the left side was audible in the right ear, and vaguely localized on the right side. At 90 cm., for instance, the fork was thus

[1] More, *op. cit.*, 315, 316.

heard. In this case the air wave had to go around the head. As the fork was now brought nearer the deaf ear, with the same intensity of vibration, the tone became inaudible for a considerable distance and until the fork came very close to the ear, when it was again feebly audible. This inaudible zone on the left side of the head was established by repeated tests for which the fork was moved in both directions, both toward and away from the ear. If now two beating forks were held at the left side of the head, one just within the perceptible limit and the other just inside the inaudible zone, beats were at once heard. These beats were localized vaguely outside the good (right) ear. That they were monaural in character is evident from the fact that with very slow beats complete interruptions of the tone were produced between each two successive beats, that is, there were intervals, as is the case with monaural beats from extremely weak tones, during which no sound was audible at all. No wandering, or side to side shifting of beats was perceived, moreover, when one fork was held at each side of the head.

It was thought that this beating might still be due to cross-conduction of the wave through the skull, so a more carefully controlled set of tests was planned. These experiments were carried out with unresonated c′ forks, 512 v.d. *J* plugged the good ear with his finger tip while the fork was held at the deaf ear. Several intensities were tried. Under these conditions no tone was heard at all even when the intensity was sufficient to make the tone audible normally at a distance of 9 or 10 meters. With the right ear plugged as just described and the stem of the fork held against the skull the tone was at once perceived, now located definitely in the right ear. It became inaudible when the tone became so weak as not to be heard by the normal ear at a distance of 60 cm. This intensity, with the opposite (good) ear plugged, it should be noted, is considerable. The measurement is important, as it is impossible on normal subjects. Let it be noted also that *J* is an usually reliable observer. In this measurement the stem of the fork was held against the mastoid process behind *J*'s deaf ear. When the right

ear was plugged and both forks at a slightly different pitch were held before the deaf ear no beating was heard whatever. This is significant for, as has been said above, even inaudible tones may be heard to beat. If now one fork was held with the stem in contact with the mastoid process behind the deaf ear there was still no beating though the fork was itself audible. Beats were at once remarked when the second fork was also held against the skull. No variation in these results was obtained when the tones were conducted separately, one to each ear, either in tubes or along rods as described in the early pages of this paper, *i. e.*, the results were like those obtained by holding the unresonated forks to the ears. Two tones thus conducted to the deaf ear were inaudible and did not beat.

These experiments with a monaural subject, then, agree in a striking manner both with those conducted by More under different methods and with those here reported on normal observers. In every case the results are directly against the view that binaural beats and the perception of phase differences are to be explained on the basis of conduction of the sound wave from ear to ear through the skull. We seem to have no choice but to conclude that binaural beats are distinct both in character and in physiological cause from monaural beats. The latter are more discrete than the former, do not wander or shift from side to side, and may involve complete interruptions in the audibility of the beating tones. If the tones conducted separately to the two ears are unusually intense there may possibly be interference in each ear due to bone conduction of each tone to the opposite ear, where it will interfere with the other tone. In such cases beats are of a dual nature, being partly monaural and partly binaural. Such cases, if they ever occur, are extremely rare. Pure binaural beats are evidently not beats at all in the usual sense of the term as used in acoustics; they are periodically perceived changes in a tone whose location wanders or shifts from ear to ear. As these shiftings become rapid the effect is that the perceived changes in the tone approach more nearly the character of true monaural beats, and may be counted with more precision and certainty.

Binaural beats and the perception of phase differences are evidently to be explained on the same principle. Both of these phenomena seem to be cortical in origin, while monaural beats probably originate in the basilar membrane. Combination tones, both 'subjective' and 'objective,' are in all probability objective in origin to the basilar membrane. For the production of 'subjective' combination tones both waves must operate in the same ear, where each periodically modifies the intensity of the other.

The conclusion to which we are here driven by facts, that binaural beats are but the shifting of the location of the sound from side to side, and are not true beats, may not, after all, violate so seriously as Wilson and Myers suppose our present conception of the nature of the nerve impulse. It is well known that the mere passage of an electrical current through the nerve fiber does not excite it. Only changes in the current, whether increasing or decreasing it, excite the nerve. Up to a certain limit the more sudden the change the greater the effectiveness of the stimulus. Oscillations in a very intense current will continue to excite the nerve up to a frequency of 1,000,000 per second.[1] The nerve fiber has a very short 'refractory period' during which it is inexcitable by another stimulus which follows the first. The length of this period varies with different nerves, and seems not to exceed .002 second. Just how short the period may be in a sensory nerve such as the auditory nerve, or rather the fibers of the cochlear branch of this nerve, is not easy to determine. There is evidence, moreover, that the refractory period of the nerve is conditioned by central rather than by peripheral factors.[2]

There seems, then, to be ample basis in fact for a theory that the perception of phase differences at the two ears is due to some central factor. Let us recall that visual perception of distance and direction rests on numerous impulses not directly in consciousness; that these several impulses in many cases have no peripheral connections whatever and

[1] Ladd and Woodworth, 'Elements of Physiological Psychology,' p. 131. References are here given.

[2] *Ibid.*, p. 165.

come to the cortical centers through different sense channels; and that the organism by the trial and error method of procedure gets gradually and unconsciously to 'take note' of them in its responses, thus learning to locate objects. It is not inconceivable that by a similar process the organism comes gradually in its responses to take note of certain central differences in the 'phases' of the streams of impulses from the two ears. It is hardly necessary in our present day in psychology to remind the reader that the organism as a whole is after all the responding or reacting agent, not consciousness. This view is consistent with our present behavioristic tendencies. In fact I see no alternative but to accept some such a theory of central perception of phase differences as here suggested. Our theories must follow the facts. At present this view does not seem to be incompatible with any of the more general theories of hearing whatever the mode of analysis in the ear may be assumed to be.

In closing I wish to express my thanks to the instructors in the University of Minnesota who served as observers, and to Professors James R. Angell and H. A. Carr for their assistance in carrying out the experiments on the monaural subject.

THE INTELLIGENCE EXAMINATION AND EVALUATION

A Study of the Child's Mind

(Second Report)

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I. Introduction

In this second report on the Intelligence Examination and Evaluation, I shall try to take up the various faculties of the child's mind, not in a critical but rather an interpretive way, and explain quite fully the method of examination and evaluation by means of the tests and test-sheet published in my previous paper.[1] This study I trust will not only serve in the testing and understanding of the normal child,[2] but will especially give the psychoclinical point of view—that is, that of the trained physician who is likewise a trained psychologist—for the discernment and comprehension of the atypical and abnormal one.

Our criticism in the previous paper, based upon extended theoretical study and practical experience, led to the conclusion that the Binet method is utterly inadequate to furnish one with either an accurate or truthful equation of the general intellectual ability of an individual, and that, furthermore, an equation of the *general* ability—even if that were obtainable by this method—is of no diagnostic value whatsoever to the psychopathologist or psychoclinicist, for in mental pathology disease processes involve not the *general* ability (neither the brain generally nor the mental

[1] 'The Intelligence Examination and Evaluation and a New Intelligence Examination Sheet,' *J. of the A.M.A.*, July 31, 1915.

[2] Our revised sheet also contains more difficult tests, so that the sheet may be used in examining adults too.

functions generally) but indeed operate very electively—at times most astonishingly so.[1] Drugs and poisons act similarly. Besides, in that group of cases known as *psychopathic constitutions*—that group which is largest in number, most important because most amenable to treatment, and most interesting because it embraces many ingenious personalities or individuals of talent, so many, in short, who, be they more or less (*or not at all*) intellectually deficient or defective, are free individuals in their community or in society, with strong potentials for good, and especially for bad, whose life tenor from the earliest may be seen winding both in and out the highway of normality, and who are not aberrant by reason of affected intelligence, but because their leverage of mental functioning is out of gear—in this large and immensely important group, Binet tests bring out nothing and test nothing, and merely mislead to false conclusions.[2]

Our mental and intelligence examination, if it is to be of practical value, must supply us with data that can serve in diagnosis, prognosis and prophylaxis, and that shall give us a clew to therapy and remediable pedagogic procedures. It is readily to be seen that an equation of the general intellectual ability (again to say, if this were obtainable) can of course in no wise fill this requirement, not even approximately. With our procedure (and sheet) no such equat on is therefore sought; we test separately the so-called faculties of the mind and several additional psychological processes influencing these faculties, and scrutinize singly the data obtained before summing up, just as the physician must note his findings in examination of heart, lungs, kidneys, etc., etc., singly before collating. For only by keeping the condition of these individual organs *disjointly* in mind can he finally survey his case *jointly*, and come to a diagnosis.

Our method, then, does not methodically apply any *scale*, nor give you any *general* grading; it affords one, after the application of the tests, an understanding of the tested individual's memory, knowledge, comprehension, combi-

[1] See G. Anton, 'Jurist.-psych. Grenzfragen,' Halle, 1910, p. 5–6; also Th. Ziehen, 'Die Geisteskrankheiten des Kindesalters,' Berlin, 1915, p. 69–70.

[2] For the literature on these types see preceding article.

national ability, and finally his attention, feeling, and reliability of memory.[1] If his memory is found poor, it may explain the lack of knowledge in such individual, for knowledge is only possible with a normally-functioning memory. If attention is strongly involved, this may explain a poor memory; for, in order to remember, the attention must hold long enough to have the process of memory register a series of concepts. Or again, focusing clinically, if a separate faculty, as that of combination, is found defective, we immediately think of one of those psychoses (as dementia præcox) in which this faculty is the first to show invalidation, etc.

But mental pathology has taught us the necessity of even refining our examinations more minutely, of testing memory, for instance, not grossly, but as to kind and time; *i. e.*, immediate, recent and past memory, memory of the heard, the seen, of language, numbers, color, form, etc.; of testing comprehension, not as such generally, but the comprehension of concrete ideas, abstract ideas, of numbers, differences, time, form, pictures, etc. In the Korsakoff syndrome,[2] for example, in which the patient seems to have lost all sense of time, place, and recent events, careful study has elicited the following: The sense of time is in no wise impaired, notwithstanding how evident and positive this appears on clinical observation; there exists, however, momentary disorientation, the patient remembering nothing or little of the recent, though past memory and the knowledge and experience derived from that memory remain quite normal.[3] Things told him at the moment can be repeated (*i. e.*, his immediate or rote memory, or, as one might term it, *impressibility* of memory, is intact), but the minor associations which go with such immediate registrations and localize them in time, and which are so all-important for our momentary orienta-

[1] Out of which stuff the intelligence is made. (See previous paper.)

[2] A chronic mental condition (mostly, though not always, due to chronic alcoholism) characterized by disorientation, irregularly impaired memory and confabulation, commonly associated with multiple neuritis.

[3] But there may also occur a retro-active amnesia, going considerably beyond the period of the illness itself.

tion, are not retained; and hence, too, all idea of sequence is lacking. To cover over these immediate and recent memory lapses (as well as the time involved in the retroactive amnasia), the Korsakoff patient fabricates prodigiously —confabulates, as we say; and these ever-changing confabulations serve him for pseudo-memories and save him embarrassment, and leave him, in fact, remarkably unconcerned.

The interesting studies on this syndrome by Gregor[1] in recent years also showed that through continuous repetition and practice the impressibility may be increased and 'widened,' and thus the memory defect improved.

Here, then, you will observe how through finer and more detailed memory examination and study, the patho-psychology of this psychosis was unravelled, and even a helping therapy found.

Other examples could be given, as that of paresis, or the illuminating aphasia group. But the above will surely suffice to confirm that crass testing will never evolve fine criteria to diagnosis, and that only by fuller and *detailed* examination will one ever clear up complicated entities, or come upon clews to therapy. Are we sure that the pathology involved in the case of a child that does not 'get on' in school, or the one brought to us with the home-made Binet diagnosis of 'defective' or 'feebleminded'—the diagnosis that every college girl now attempts, and that is being made in all our clinics, and at the children's courts, chiefly by non-medical psychological workers (and some equally well prepared medical ones, too!) who have had no training in mental pathology, and just picked up their knowledge as opportunity shuffled them along—are we quite sure that this pathology is really simple? Have enough medico-psychologically trained specialists applied themselves to the problem, and sufficiently, to maintain anything? Have we, besides, chosen the proper guides (do we even admit of such), grasped the right tools, mapped out reputable paths of procedure?

[1] "Beiträge zur Psychopathologie des Gedächtnisses," *Monat. f. Psychiat. u Neurol.*, Bd. 25, 1909; also "Beiträge zur Kenntnis der Gedächtnisstörung bei der Korsakoff'schen Psychose," *Monatschr. f. Psych. u. Neurol.*, Bd. 21, 1907; also Bd. 23, 1909.

Let me stop long enough here to dwell upon this point at issue, this point of sincerity in work and legitimacy in method, for upon it rests the future of our subject, its honor or dishonor, and whether it is to be esteemed a science, a pastime, or a dubious profession.

Reviewing this field of endeavor and its literary output, one notices at a glance that most of the energy expended has come from non-medical sources, and that it is chiefly Binet-imbued and statistically tempered. One readily notices also that neither pathology nor the individual is conspicuous in the background or visible at hand; *the test's the thing*. And the test is applied to hundreds of individuals, until the idea of test has become hypertrophied in the examiner's mind—and then we see single supposedly specific tests applied a thousand, nay even several thousand times to *test* that *TEST*, and prove it specific. In this way the child's mind at best becomes for these investigators a form into which a test must fit or misfit. The very fact that the test is the thing focused upon, and not the individual mind with its countless variations in functioning, makes the results, even where gained from ten thousand tests, diagnostically valueless.[1]

I have seen the most glaring blunders made in diagnosis, even to the extent of advising children to be sent to Randall's Island, though there was no mental defect whatever present in the case, but only a remediable retardation—because the work was done 'with a Binet' by an unskilled lay worker, and the physician in charge took the word of this worker for it. Again, of late I saw a child diagnosed as a microcephalic imbecile (and Seguin exercises prescribed for it by way of treatment) whose queer facies made me spot it outright as some atypical secretory dystrophy (*i. e.*, the trouble being due to some disturbance of internal secretion). On examination *I found its scrotum empty*, and a state of infantilism present—possibly here a case of genito-genic infantilism (no extended study could be made). This patient had been

[1] A very interesting paper that I have come upon since writing the above points this fact out very plainly—'Difficulties in the Interpretation of Mental Tests—Types and Examples,' by Frances Porter, *The Psychological Clinic*, October, 1915. See also Bobertag's comment on the test of "defining," under Comprehension.

diagnosed by a non-medical lady clinician, and all with a Binet!—although the great interest and importance of the case, as well as the urgent indication for therapy, lay in the absence of the sex glands, the mental abnormality being quite secondary. Of course, no one would deny the child's being imbecilic and probably microcephalic. But what about pathogenesis? Shall we ever get beyond the amateur stage and graduate into the scientific, if no more than this is demanded at our clinics? For is there not latent in every such (mentally abnormal) case, the possibility of a pathology which only the physician can interpret,—syphilis, ethmoid disease, tumor, hydrocephalus, serous meningitis, etc.?

On the other hand, how many physicians have really been trained in the psychological fundamentals of this work or have aided in its advance? Why does its story, in America, comparatively all come from the lay pen, either the psychologist or educationalist? Why has not the physician discovered the flaws in the work and pointed them out to the profession? Why do even these facts come from the non-medical workers?[1]

No reproach, of course, falls to the psychologists and educationalists. On the contrary, all praise must be theirs for their activities. And should our work here ever attain the stage of excellence of that abroad (at Berlin, Leipzig or Münich, for instance) the glory will be theirs, for their eyes were open and they saw, or at least tried to see, when our entire medical profession was still disinterested or asleep.

In this very city we have a hundred organizations, federations, parent associations, bureaus, etc., devoted to the study and advance of the child, mentally, physically, ethically, socially, and to the uplift of the parents in behalf of the child, with leaders who give lectures, readings, and hold conferences—all of them amateurs—and call in from time to time, or for a series of talks, other amateurs or self-assumed "authorities," or, again, some really prominent social workers, psychologists and other eminent individuals. They not only discuss the child, abnormals, remedial pedagogics, etc., but

[1] Miss Porter's article, just mentioned, is an example again.

in some circles eugenics, the sexual and the psycho-analytic as well. Besides, as already intimated, at our clinics, lay-workers have quite usurped the field.—But where is the medical college in the United States that has a *physician* devoted to the non-pediatric study of the child, the study of clinical psychology, or juvenile research (or, as it has also been termed, pædology, which embraces the many methods of therapy besides), a study scientifically pursued, with respect for precedents, a regard for authorities, an inkling to thoroughness—*based on the fundamentals of physiology, pathology, psycho-pathology, etc.?* And at what university is this subject really thought serious enough to teach, with lecture or clinic, and scientifically, on the above basis—not merely down in the catalogue! *but really taught*—and where a laboratory working in its behalf? And if the answer is *nowhere*, then this so-called Decade of the Child is, at least with us, a superficial, amateur, bungling decade, and the profession of medicine is verily losing some of its most precious heritage to the educationalist not having sense enough to come into its own.

II. Knowledge

Knowledge is made up of facts deposited in memory, deposited and retained; as they are accumulated they must be deposed and arranged *in order*, the facts being grouped and subgrouped. These groups again must be inter-linked through associations. Only in this way is knowledge reproducible, facilely reproducible. (Mere re-cognition of a thing is not knowledge unless one can associate it with some other concept, and again differentiate it from others. In a way, "knowledge is differentiation." We know a thing [a horse for instance] by knowing how it differs from other things [how it differs from a cow, zebra, etc.[1]]. This involves us in the matter of comprehension—and in truth, knowledge is based on comprehension, just as it also is on memory.) The

[1] These groups and subgroups are gathered by way of association, according to similarities; hence classes, categories, etc.—hence our word "assimilation" (ad-similare). but they are also grouped and subgrouped because of variations from such similarities; The processes of assimilating and differentiating are constantly taking place.

No.	Age.
Name.	Class.
Date. (1)	1st Examination by
Date. (2)	2d " "

1. **Knowledge**: (retention)

(*a*) School report: reading, writing (c. d.), arithmetic, geography, history.

(*b*) Experience, etc.:

Name, mouth, etc., *at 3*. Sex, pocket things, *at 4*.

Colors *at 5–8* [R. B. G. Y. - Br. Gr. V. P. O.].

Things in a room, on the table, goods, metals, relations, mother's home occupations, etc., *at 6*.

Money (4 to 6 coins *at 7;* all coins and bills *at 10*).

Things on the street, engines, trains, ambulance, subway, *at 6–7*.

Winter, spring, harvest, Fall, Thanksgiving, Christmas, *at 6–7*.

How does one get wood, milk, honey, ice, water, food, shoes, heat, gas, music? How is a river formed? (*at 12*).

Of what is bread, flour, butter, paper, woolen goods, silk, made?

Central Park, Bronx Zoo, Brooklyn Bridge, etc.

Days of the week, *at 9*, the months, *at 10*.

[(*c*) Words: 50 in 2′, *at 9;* 60 in 3′, *at 11;* 3 rhymes in 1′ *at 11–12*.]

2. **Memory**:

(*a*) I. I go to the store and buy . . . . . . (4 to 12 facts).

II. 138, 692; 5734296 III. Recalls (I).

(2 or 3 *at 3;* 3 *at 4;* 4 *at 5;* 5 to 6 *at 6–7;* 6 to 7 *at 8;* 6 to 9 *up to 12*.)

(*b*) Optic memory: I. *f.m.* II. □ *at 5;* △ *at 6;* ◇ *at 7;* *at*

(*c*) Color " I. *f.m.* II. *at 4–6*. III. naming, *at 7–*

(*d*) Repeats sentences:

(6 syl. *at 3;* 10 *at 5;* 14 to 26 *at 6;* 16 to 26 *at 7–8;* to 30 *at 9;* 26 to 40 *at 12*.)

[(*e*) Remembers from paragraph read: (2 facts *at 8;* 6 *at 9;* 7 *at 12*.)]

(*f*) Story; I. repeats II. exactness III. following visit

(*g*) Of yesterday: Last Sunday: Past: Immediate:

(*h*) Carries out commissions (3 *at 6*) . . . . . .

3. **Comprehension**:

(*a*) Numbers: (4 to 6 pennies *at 5;* 13 *at 6–7*, stamps 111222 *at 8;* change *at 9*)

Which is greater 6 or 10; 100 or 50; 100 or 500?

(*b*) Isolation: Complexion: Generalization:

3. COMPREHENSION:—*Continued*

(*c*) Differentiation:

Hand—foot	steps—ladder	fly—bee	*at 8*
Ox—horse	wood—glass	water—ice	
Butterfly—bird	paper—cloth	child—dwarf	
	Miser—one who saves		*at 13–15.*
	Lawyer—judge		
	King—president		

[(*d*) Fork, chair, hammer, cake, doll, cab, horse, soldier, penny, rose.]

(per usage *at 5–6;* description; per genus *at 9.*)

(*e*) Abstract ideas: ungratefulness, envy, bravery, justice, charity, *at 11–12.*

poverty, misery, evolution, revolution, advent, event, pride, *at 15–*

questions of logical causal relations, *at 13–14–*

(*f*) Ethical ideas: good, bad, wrong, sin, *at 10–11.*

Mistake-lie borrow-steal lend-give *at 12–13.*

Exaggeration, deserved and undeserved punishment, fraudulence.

What should one do if . . . .

[(*g*) Time: morning, afternoon, earlier, later, yesterday, day before, to-morrow, day after, etc., *at 6.*]

(*h*) Form: *at 6.* (*i*) Space: I. and II. *at 6–7.* III. *at 13–* .

(*j*) Directions: *at 6–*

(*k*) Weights: (arranges 2 *at 5,* 5 to 6 *at 9.*)

(*l*) Pictures: (substance stage *up to 8;* action *8 to 10;* relations *10 to 13*)

(enumerates *at 3;* describes *at 7;* interprets *at 15.*)

Physically:
Senses: S. H. T. S.
Speech:
Sensory-*motor:* (agility, dexterity, etc.)

Diagnosis (clinical):
" (psychological):
Mentally:

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4. COMBINATION:

(*a*) Orientation: (time, place)

(*b*) Geographical sense: (how get to . . . . . . from . . . . . .)

(*c*) Arithmetical problems (see 1 (*a*)).

$5 + 6 =$ $32 - 11 =$ $18 \div \times = 6, \times =$

3 loaves among 2 boys? Months in $\frac{3}{4}$ year?

If 3 eggs cost 20 . . . . . . I am thinking of a number. If I add 5 I get 12 . . . . . .

(*d*) Completion tests:

I. Ship-storm-lost. New York-money-river. (Mountain-lumber-wealth.)

(in 2 sentences *at 10;* in one sentence *at 11–*)

II. Sky-red. Sun-midday. Snow-muddy. (Drinking-poverty.)

4. COMBINATION:—*Continued*
III. Put dissected sentences together (B's 1, 2, 3) *at 11–*
IV. Though it is raining . . . . Even though the soup was burnt . . . . Even though I am ill . . . .
V. Compose a story out of: winter-night, soldier, bitter-cold, froze, relieved, Death, *at 13*.
(*e*) Part-picture test: I. (animals) II. (blocks) III. (B's) *at 7*.
(*f*) Many cooks. . . . People who live. . . . He who digs. . . . Changes mind. . . .
(*g*) Grasping the "point" of a story *at 7*. I.
II. (4. (*d*) V.) *at 15–* III. Interprets picture (3. (*l*)).
(*h*) Critique: I. Exaggeration II. Absurdity (B's 5) *at 11–*
III. Of personal acts.
(*i*) Problems in judgment, etc. I. B's 1st series *at-10.*
II. 2d series *at 11–12–* III. Cutting paper *at 13–*
IV. Reversed triangle *at 13–* V. De Sanctis' No. 6 (*a*): (*b*,) *adult:*

5. ATTENTION:
(*a*) Scope: I. Observation in room: II. In picture:
III. Exposure test:
(*b*) Concentration: I. Reversed association (7 2 9 *at 6;* 20–1 *at 8*). days, months, name.
II. Simultaneous addition: III. Counting syllables:
(*c*) Tenacity: I. Cancellation: II. Assortment: III. Dots:

6. FEELING:
(*a*) What good deed have you ever done?
(*b*) A sparrow had a nest. . . .
(*c*) Example of want (with theft), cruelty, joy, etc. (note comment).
(*d*) Aesthetic choosing - 1, 2, 3, (of Binet's) *at 6*.
(*e*) Aesthetic judgment of pictures *at 10–12–*

7. RELIABILITY OF MEMORY: (suggestibility, etc.)
(*a*) Exposure test:
(*b*) I. Picture II. (3. (*l*)) III. Recognition test:
(*c*) Auditory (news account):

[These sheets may be obtained from the Fetzer Press, 62 Reade Street, New York.]

greater the number of associations the more readily the ability to get hold of the desired fact—and the more apparent the knowledge.

Our accumulations of facts are multitudinous. To test such inventory, therefore, one could not catalog it all. One can, however, test important deposits here and there, and thus roughly judge the nature of the whole, for our training and education are in a way much the same, and the com-

munity to which we react is also for most of us the same. When this is not the case we may have to examine very differently and make ample allowances. The child from the country, for instance, has gathered different knowledge from that reared in the city. Still greater is the difference when the child comes from some foreign land.

In testing this *Knowledge*, we must bear in mind that we stand on rather problematical ground, and must know how to be both discerning and charitable. There is school knowledge, attained at school, and that gained through experience (picked up at home, on the street, from others, etc.). A child, after all, can only know that which it has been 'exposed to,' so to speak. As it grows older, it does not by any means retain all it has gathered. By the time it gets to adult life it has long since forgotten an astonishing lot.[1] If the 'educated' were tested, writes Scholtz,[2] much shocking ignorance would come to light, for, indeed, we retain only that which we use in our daily life, that which we hold through much repetition, and that in which we are especially interested. We will be careful, therefore, and especially in adults, to make no final or indelible appraisement until we have carefully considered the facts, weighed all 'extenuating circumstances,' and, when necessary, made liberal concessions.

The Tests for Knowledge

Our procedure is as follows: first of all (*a*) we note the school report (obtained from the teacher) of the child's standing in reading, writing (from copy and from dictation), arithmetic, geography, and history, for the teacher who sees

[1] See Rodenwald's study of 174 recruits. *Monatsch. f. Psychiat. und Neurolog.*, Bd. 17, 1905. He found that there were hundreds of facts you would expect a man to remember that had long since slipped out of his mind. Even the simplest questions were at times misanswered.

Similar remarkable results were obtained in testing lower Italian Folk-classes. The tests showed that these people live apart from all such world interests as politics, religion, culture, etc. See Pasta Lombroso-Carrara and Mario-Carrara, Turin, 'Aus der Vorstellungswelt des niederen Volkes,' *Morgen* (*Wochenschr. f. deut. Kultur*), No. 13, 1908.

[2] L. Scholtz, 'Anomale Kinder,' Berlin, Karger, 1912, p. 102–3.

the child day in and day out gets a very fair idea of its various abilities. (We may use the letters E [excellent], G [good], F [fair], P [poor], and X [failure], in checking the facts.[1])

Under (*b*) we probe knowledge gained through experience, and must needs give a considerable number of questions to determine this. No doubt much of the knowledge thus brought out is taught at school, or at least touched upon there. However, there is no necessity of indicating any sharp dividing line.

We begin by asking the child for its family name, then have it touch its mouth, ear, eyes, nose, etc. (in Binet's way), and ask whether it is a boy or a girl; we also have it recognize pocket things, namely, handkerchief, key, knife, penny.[2]

The number in italics, to the right of the tests, gives the approximate age at which such test should be passed (*i. e.*, the age at which these facts should be known or answered). *Where a hyphen follows such number, it indicates that the question may be normal even at a higher age.*

Concerning *colors*, much divergence of opinion exists among authorities as to when they are normally learned. Some give the age as early as four, others as late as eight. Red, blue, green and yellow are first learned, and later brown, gray, violet and orange. According to Ziehen[3] (and Meumann) the normal child knows the chief colors, inclusive

[1] It will be well to compare the school opinion and standing with our own results at the end of the tests. It may be remembered, however, as Stern points out, that the school work (and marks and advancement) depend not only on intelligence, but on facts that have nothing at all to do with intelligence, but belong to the domain of will; *i. e.*, intensity and perseverance of attention, diligence, conscientiousness, sense of duty, etc. Stern believes that, because of these facts, the intelligence examinations have shown a very large number of children to be pedagogically graded below their 'intelligence age' (according to Binet). See W. Stern, 'Die psychol. Methoden d. Intelligenzprüfung,' etc., Leipzig, 1912, p. 39, et seq. Stern also points out (p. 79) that spontaneous intelligence cannot be gauged by tests, and is therefore excluded. So we must seek to estimate it through a criterion that lies outside the experiment. As such the teacher's appraisement of the pupil offers itself.

[2] Terman and Childs place the knowledge of sex and pocket things in the third year. For the knowledge of the normal child up to its third year (from the first to the 1000th day), see W. Preyer, 'Die Seele des Kindes,' Leipzig, 1905 (6th edition), p. 399.

[3] Ziehen, 'Die Erkennung des Schwachsinns im Kindesalter,' Berlin, 1909, p. 24.

of black and white, at the end of its third year. But green and blue, brown and gray, may often yet be confused by the normal child in the fourth year, exceptionally even in the fifth. In the feeble-minded knowledge of colors comes much later, and may be defective, while the imbecile may never gain a conception of colors, or only the most primitive. Gaupp[1] gives the average age for color-knowledge at six. A study of Warburg's[2] testing 1,800 children showed that color *naming* might be used, in a measure, as an intelligence test, inasmuch as the lack of color naming in this series of tests went parallel with intellectual defect. Binet's test of the four fundamental colors in the seventh year, is of this type. Bobertag found that only a half of the seven year olds can pass the test. (But see under memory.)

In testing '*things in a room*,' I show a picture of a room containing chairs, table, mantel, lamp, wall-pictures, etc., and ask: "What things do you see in this picture?" One may also ask the child to point out a chair, picture, etc., in the room it is in. A child finds this much easier than *naming* a thing you point to (in a picture or a room). *In all naming tests more mistakes are made than in indicating tests.* Ziehen remarks (l. c., p. 57) that the word may be missing, although the idea (Sachvorstellung) be present and understood. '*Things on a table*' are knives, forks, spoons, etc. By 'relations,' sisters, brothers, aunts, uncles, and cousins are meant.

As to money, much again depends upon whether or not the child comes or has come in contact with it. In eliciting this knowledge, show a cent, nickel, dime, quarter, half-dollar, etc.

[1] R. Gaupp, 'Psychologie des Kindes,' Teubner, Leipzig, 1910, p. 59. Much depends, he says, upon whether colors are shown and the indicating of colors (Farbenbezeichnen) practiced. How much in fact depends upon the constant *re-seeing* of colors and hearing the names of such, is shown in the case of a child who at seven lost her eyesight, regaining it again at 17. In the meantime she forgot all her color knowledge, and at 17 had to re-learn almost as she did in the beginning. O. Heyfelder, 'Die Kindheit des Menschen,' Erlangen, 1858, second edit., p. 13. O. Bobertag points out that there is a very great difference in the age of color knowledge according to whether a child is of the lower or higher classes, also between males and females, the latter being more proficient. 'Über Intelligenzprüfungen nach der Methode von Binet u. Simon,' Leipzig, 1914, p. 89.

[2] *Münch. med. Woch.*, No. 49, 1909.

By '*things on the street*,' horse, wagon, car, policeman, letter-carrier, soldier, etc., are meant.

The following facts (see next line on intelligence sheet) may also be brought out by inquiring "When does one use sleds?" "When does the snow fall?" "When does the grass come up?" "When do the leaves fall (or turn red)?" "When does one gather in the hay, fruit, crops?" etc.[1]

The succeeding lines contain questions of varying difficulty, and no note of age is made, it being as yet undecided when a child ought to have this knowledge. One may ask older children or adults still more difficult questions, *i. e.*, "Of what are houses made?" "What is the horizon?" "What is a shadow?" The meaning of "oriental," "civilized," etc., ('taxes,' 'insurance,' 'jury duty,' 'preparedness'). Often enough there is no exact, not even approximate age at which one can say these facts become known to a child (reading, school instruction, etc., having much to do with it). One learns considerable, however, of the mental ability, from the answers given. One should constantly bear in mind that lack of knowledge does not always mean mental inability. Ask adults, for instance, "Where does bronze come from?" "What is soap made of?" "Cheese?" "Linen?" "What's a tide?" "Light?" "The Aurora?" "An atom?" etc.[2]

We next have the child tell about Central Park, or the Bronx Zoo, etc., and note the way it describes what it has seen, what *animals* it knows, etc. I also inquire how it gets there, to learn its sense of orientation—which fact may then be noted under 4 (*a*).[3]

[1] The question, "What is autumn?" sometimes proves a sticker. "Thanksgiving" may be too difficult for children not learning of it in school. One of my students first found it correctly answered (tested on orphan asylum children) at nine. One may also ask "Why do we celebrate the fourth of July?" How bread is made should be known, according to Cimbal, at 10.

[2] And both Rodenwald and Ziehen (l. c.) point out that normal adults may even go wrong on questions like "On what river does the city lie in which you live?" "Who was the last mayor, governor, etc.?" "Who is in office now?" *Especially if frightened*, an individual will go wrong on questions like this. *If the individual being tested is frightened, you can place no value whatsoever on your examinations if the results prove negative.* This is an urgent lesson for the courts to learn.

[3] Under the tests for combination ability.

One finally inquires as to the names of the days of the week,[1] and of the months (the former, according to Binet, known at nine, the latter at ten). The date, as to the month and year, is also known at nine, but Scholz warns against asking for the date of the day, as adults often cannot answer this.

As to (*c*) *words*[2] (and language), one must also here know how to make allowances. Some children are rich in words through natural talent, some through much reading, some through a good home, a governess, etc. Children rich in language 'pass off' splendidly; yet such children are not necessarily superior in intellect to those who have a poor verbal store. Mehnert at the Dresden Congress[3] alluded to two children he had seen at two different schools (of different social standing), the first being brought to school by its governess, the second coming alone and with the house key tied about its neck. And he commented: "How many more practical duties that latter head fulfills and is capable of than the child of the richer class, who in the simplest practical urgencies may stand helpless. Must we not take this into account in the mental evaluation?"

There are most capable laboratory workers who conversationally are startlingly uncouth. Again, there are artists whose language ability is of the most primitive type, and who could not for their lives tell a connected story, and yet, at their canvas, with crayon or brush, their fantasy veritably becomes creative and alive. Dohrn[4] intimated

[1] In order to be sure the child does not spin these off automatically, have it begin with "Wednesday" and so on to "Tuesday." Also ask, what day was the day before yesterday; what day will be tomorrow.

In Bobertag's tests, 75 per cent. of the 8-year-olds knew the days of the week; hence, he believes the test ought to be placed in the eighth year. Asking for the date, however, he does not approve as a suitable test, and the naming of the months is likewise questionable (depending on practice).

[2] Bobertag (*l. c.*) points out that this test of giving 60 words in three minutes is found decidedly unpleasant even by adults, and that really intelligent children may fail on it. He thinks it of no use as an intelligence test. A much greater number of words can be found by the examinee if you lead him, directing what groups of words he should choose; thus 'things indoors, outdoors, flowers, animals,' etc.

[3] Deutscher Kongress für Jugendbildung und Jugendkunde, Dresden, Oct., 1911. Report pub. by Teubner, Leipzig, 1912.

[4] At the Dresden Congress just mentioned.

that there may be children of this same type, poor in words, in language, but eloquent enough could they express themselves with line or color. *This, too, is intelligence.*[1] We might also recall to mind Oliver Goldsmith, 'who,' as Garrick penned, 'wrote like an angel, but talked like poor Poll.'[2] There are many children, and adults too, who, like him, are too shy, or too 'inhibited,' or too nervous to speak—who can never speak well, especially not on 'occasions' (for just then they get particularly nervous or panicky)—and yet can *write*, even with exceptional skill. Again, some 'temperaments' are even brilliant in certain surroundings and almost mute in others, or, as Washington Irving tells the story somewhere, one day a nonentity at the foot of the table and on the next at its head, and its very life.

On the other hand, there are children (and adults) very glib of speech, ever ready with an answer, with so many answers, that conversationally they appear most intelligent, but who, on careful scrutiny, show themselves very superficial, who never have their ideas sharply focused, and whose answers, as Scholz puts it, never taper to accuracy. These are the l'esprit d' à-peu-près of the French.[3] In the adult this type must be distinguished from the neurotic individual who has an impellence to speech and little inhibition to stop, and whose restless and awake nature causes him to read much and be informed on every *new* or scientific subject (most superficially, of course—or not informed at all!). In the hysterical the same thing occurs, only these individuals also wish to be in the lime-light, wish to be admired and

[1] Not long ago I heard an artisan cross-examined about the mental ability of one of his children—a child evidently not of the resplendent kind—say, "Everybody can't be a school teacher, everybody isn't smart that way; but one can be smart with the hands too. My one boy is a mechanic, and very good, and the girl was two years at jewelry work. Taking proper care of a home and doing everything in it is also smart, isn't it?" And this is true. Who knows if the schooling of such children (his children, through change of domicile, had gone from school to school, and were part of the time in some charitable institution) had been ample and continuous, they might not have been quite up to the shining mark of other children of that age?

[2] The estimate formed of young Goldsmith by his contemporaries at school was that he was a dull boy, 'a stupid, heavy blockhead!' (William Black in 'English Men of Letters Series').

[3] See Scholz's excellent description of this type; 'Anomale Kinder,' *l. c.*, p. 108–9.

esteemed, and will argue on every topic under the sun, and never give in. They are terrible individuals to have to meet socially, and leave alive!

As to the test of *rhyming*, the last in this rubric, it is not an important one, for rhyming may be a matter of special talent and not of intelligence. Besides, the nervous child may at times be unable to hit upon a single one in your presence, yet at home can find a dozen. Bobertag noticed that some children could not quite grasp what was wanted of them, and again, others, who understood and were intelligent enough, could not come upon the word. One would scarcely say, he comments, that this failure indicates a lack of intelligence.

III. Memory

Before taking up the tests of memory, certain psychological data may be advantageously reviewed for the sake of brief orientation and for the better understanding of the irregularities we are likely to find in both our normal and abnormal charges.

In the process of perception we assume[1] a physiological activity or change in the cortical cells stimulated, leaving its trace in a nigh permanent alteration, either in the disposition of the molecules or through a discharge of their ions, or a metabolic generation of a kind of *residuum*. These cells thereafter are predisposed to the same excitation, and should such again come to them, the former presentation again becomes conscious. This alteration, this *residual something* is memory.

Yet not singly but in complicated clusters are these cells originally stimulated, depending upon the various and manifold perceptions occurring at the same time; and ever after, when any one set of cells is innervated, a hundred others will be consorted and set vibrating and many series of memories become conscious. Hence, our association of ideas.

Here, then, we see systems of memories grouped and relayed, not in near-lying bundles of cells, but over large areas—

[1] Following the teachings of Ziehen. See his 'Das Gedächtnis,' Berlin, Verlag Hirschwald, 1908.

circuits, we might think them, connecting with other circuits. The same cells or groups may be used in several or even in many circuits, so that from such 'cell station' one could start off on different association tours (from the association 'John,' for instance, we might start off on 'John Brown,' or 'John Bunyan' or 'King John' or Shakespeare's play, or maybe a friend of that name; or, through a side relay, from the name 'John-a-Dreams,' to poetic imaginative characters, or to etymology, etc.).[1]

And how actually real these large circuits are, may be gathered from the common experience that if we do not take trips over them once in a while the trail becomes obscure—may even be lost. More remarkable is the fact that in certain conditions, as hysteria (one might say the make-up of certain individuals is prone to this), whole circuits of associations are temporarily switched out at times, dissociated from these minds, so, for instance, all the memories associated with a certain event, or a certain bodily function, or the entire knowledge of a language, the mother tongue being retained. Again, through disease, circuits covering certain periods of time are sundered—there occurs an amnesia—sometimes, but transiently, to return, sometimes gone for good. Finally, through destruction of brain tissue (hemorrhage), many wires, or whole stations, may be destroyed, and great accumulations of associations blotted out (aphasia).[2] In childhood, disease processes (inflammation) may also damage the cells and connecting tracts, which never regenerate; or individuals are born with a defective brain, the cells being spoiled from the start.

There is still a remarkable fact to be noticed in the

[1] Just how we happen to go off on one or other of these associations depends upon how we are *constellated* at the time. For the psychology of this see Ziehen's 'Leitfaden d. Phys. Psychol.,' 7th edit., p. 186.

[2] When such aphasias clear up in part (as they usually do) this may occur through the absorption of blood or clots having caused pressure, or the regeneration of such cells not too severely damaged, or through other cells and fibers taking over part of the work. Again, the local destruction of nerve fibers may cause distant cells or groups to degenerate. See Monakow's *Diaschisis theory* ('Ueber Lokalisation der Hirnfunctionen,' Bergmann, Wiesbaden, 1910, p. 19). See also Monakow's larger work 'Die Lokalisation in Grosshirn,' etc., same publisher.

mechanism of memory. We spoke just now of the cells, when stimulated, becoming conscious and undergoing a change in which a latent something is left behind, so that if similarly stimulated at a later time, such cells re-awaken their former 'content'—awaken to consciousness again. This statement we must, however, modify. There are some cells which subsequently lose this quality of consciousness, and functionate without awareness. This functioning, now (we say) becomes *automatic*. Thus the feat of walking, which the child first learns so laboriously and consciously, becomes automatic; one acquires a language, and consciously works with rules of grammar, regulations, etc., and yet later speaks fluently, without a thought of the words or rules themselves—speech coming automatically. Even our first speaking, writing and reading are all learned consciously, and with heroic exertion and effort, later to become automatic. The same is true of piano-playing, bowing and fingering the violin, typewriting, dressing, eating, shaving, riding a wheel, etc.[1] We can continue these processes agoing, and have our conscious minds active in quite another direction (thus a man may walk, hum to himself and whittle a stick quite automatically, pursuing some train of thought at the same time). Automatically certain words in our process of thinking follow others (as 'either-or,' etc.)[2] and likewise one act may follow another (as when we are about to sneeze and almost at the same moment grasp for our handkerchief—sometimes even heave up with a 'thank you' before the expected 'God bless you' is given. In this latter instance we have an automatic ideational association, not only an automatic act. Such have been termed 'psycho-reflexes' (Bechterew), and quite a psychology has grown about the problem.[3] Even

[1] The opposite process has also been noted: the infant at first suckles through instinct (*i. e.*, a phylogenetic automatic act); but little by little this becomes a conscious act. See Compayré, 'L'Evolution intellectuelle et morale de l'enfant,' p. 175.

[2] And such words automatically influence and lead our thinking just as do analogies, contrasts, etc., and cause whole troops of ideas to be marshalled in one or other direction.

[3] W. v. Bechterew, 'Was ist Psychoreflexologie?' *Deutsch. Med. Woch.*, No. 32, 1912; 'Ueber die Entwicklung der psychischen Tätigkeit,' *Deut. Med. Woch.*, No. 47 and 48, 1913; *Zeit. f. Psychother. u. med. Psychol.*, Bd. V., H. II; 'Ueber die Motor-

logical conclusions and judgment may be consummated automatically, whole series of links being skipped. Our popular term for it is 'jumping at conclusions.'[1]

What has happened here? It is possible (as animal experiment has intimated)[2] that the optic thalamus takes over some of these activities, and 'runs off' sets of associations automatically, with or without the cortex participating (for in all these acts the cortex can be made to participate, the automatic act be made conscious; there is even a frequent alternating of the voluntary and automatic). At any rate, much that we have made tracks for in our brains—possibly far more than we at the moment are aware of (Bechterew's studies seem to indicate this)—once started, tends to go off automatically. Thus the recalling of addresses, fore and surnames, telephone numbers, multiplication tables, much that we 'ground' at school, old pieces, jingles, poems, music that we 'know by heart,' tunes that 'run in our heads' and songs generally, and all that we 'commit to memory.'[3] There is an unsolved mystery latent here which has not been helped a whit by the metaphysical adaptation of an artificial subconscious, or by assuming we have different kinds of memory, a rote or automatic memory, a voluntary memory, etc., for, as we have seen, they are to a certain degree interchangeable, and one may pass into the other. But we must keep this automatism and its possibilities[4] in mind, and in

ischen Assoziationsreflexe,' R. Golant, *Ergeb. d. Neurol. u. Psych.*, Bd. I., H. 3, 1912; W. v. Bechterew, 'Objektive Psychologie od. Psychoreflexologie, Die Lehre v. d. Assoziationsreflexen,' 1913, etc.

[1] See Ziehen's 'Leitfaden,' etc., *l. c.*, p. 197; E. Meumann's 'Vorlesungen,' etc., *l. c.*, Vol. I., p. 539; Störring's 'Experimentelle Untersuchungen über einfache Schlussprozesse,' *Archiv. f. d. ges. Psychol.*, XI., 1908. Such skipping of links in the association of ideas may become pathologically exaggerated, in which case we have the condition of "incoherence." What is known as stereotypy may also be thought of in connection with this mental mechanism (of automatism).

[2] Ziehen, 'Leitfaden,' *l. c.*, Chapter II. Another theory is that of Grasset's. See his 'L'hypnotisme et la Suggestion,' Paris, 1903.

[3] In fact much, if not most, of what we have automatically in mind, cannot be given consciously. To get your telephone number you 'start it' consciously, and then run it off automatically. To note the difference between this and the conscious effort, try to give the number backwards!

[4] How amazing and involved this automatism can become is seen not only in lightning calculators who show phenomenal feats of arithmetical combining, etc., but

our testing remember that some answers come mechanically, others again with conscious effort, and that some subjects, like arithmetic, may be handled by certain minds quite automatically, occasionally in the most astounding fashion. Much too, if not the most, that imbeciles 'know' is remembered automatically.

A few additional facts should also be borne in mind. Somehow, in the first two, or possibly three years, the residuum does not hold—it is almost as rapidly washed away as formed. That is, probably, why we remember nothing of this period.[1] But in the next few years what is gathered in mind is almost indelibly held there (which shows the urgent importance of taking care in these years what is fed to the child's senses, to store up in mind). There is something materialistic about it all, something more physiological than psychological; and hence, according to the kinds of mental (physiological?) "material," and, as such varies in different individuals, we have many types and variations and capabilities of memories. Yet, in the process of development, all memory capabilities are not given at once. The child of six has capabilities that the child of four has not yet acquired. As the brain matures the capabilities expand. Factors of perception and comprehension play a decided part —*form* is remembered much earlier than *time* (because comprehended much earlier), certain colors much earlier than others (the retinal rods and cones which take up certain colors presumably developing later than those which take up others), etc. Finally, when memory begins to dwindle because of disease, it goes in the reverse order that it has been acquired, and the childhood's quantum is held to the very end.

in the cases of epileptic automatic acts, and especially the reported cases of somnambulism. A drug clerk may under these conditions compound different prescriptions, point out incompatibilities, and refuse to give poisons, etc.; or another individual compose verses, or solve problems, etc., all automatically in sleep. See Loewenfeld's 'Somnambulismus und Spiritismus,' Wiesbaden, 1907. For everyday examples see Dessoir's 'Das Doppel-Ich,' Leipzig, 1896, p. 9–11; for examples in hysteria, hypnosis, etc. see Janet's 'L'automatisme psychologique,' Paris, 1889.

[1] See Compayré, *l. c.*, Ch. 6, Pt. I.

The Tests for Memory

Under (*a*) one plays make-believe, and sends the child to the store to buy several things: a loaf of bread for five cents, a bottle of milk for eight, a half pound of butter for ten or fifteen cents (and possibly adds a pickle or something for the dog). After telling the child to remember these facts, one gives it a series of numbers to repeat. Two or three series are given. In the third line will be found the number of figures a child is supposed to remember at the various ages: two or three at the age of three, for instance, three at the age of four, etc.[1] (In the space at the right of the tests, and corresponding to the second line, one notes with E, G, F, P and X, accordant with excellent, good, fair, poor and failure, how the child has passed this test. Or one may merely use a check (√) to note that the test is passed). One finally inquires of the child what it purchased at the store. To reproduce this, the child will have had to carry it in mind—to have retained or remembered it. (One again marks this result in the space to the right.)[2]

Under (*b*), optic memory, there are two tests: (I), one first puts questions as to the form of certain things, '*from memory*' (the full moon, young moon, a wheel, stamp, chimney, [pyramid] obelisk, etc.), then, (II), has the child copy certain figures, which are exposed a brief time (15′) and again covered over (for 15′). For this purpose one uses a square, a triangle, a diamond, and a more complicated figure (such as Ziehen's).

In the test of copying the square and diamond *placed*

[1] As to remembering numbers, Ziehen points out ('Intelligenzprüfung,' p. 18) that even the paretic and senile dement, and even the severest *Korsakoff* patient will carry three numbers and first stumble on the fourth. However, even Korsakoff patients may be able to retain six numbers, exceptionally even seven. But they will long since have forgotten the example under (*a*). An adult, therefore, who cannot repeat 3 or 4 numbers shows grave defect. Under such conditions simulation must also always be borne in mind. The retaining of numbers is, in the case of most people, a matter of *acoustic* memory. Bobertag (l. c., p. 59) holds this repetition of a sequence of numbers as more really a test of attention ability. Meumann appears to concur in this (*l. c.*, Vol. II., p. 260). Test (*a*) may also be varied thus: I. Give six words to hold in mind. II. Give two short mental examples. III. Now ask for the six words.

[2] The method of marking and grading is taken up below.

before it, Binet[1] found that this was accomplished at five and seven respectively; the triangle, according to Saffiotti[2] at 6. Here the test was not a memory but a talent test, the drawing remaining before the child. But Meumann appears to have employed the test (and at the same age) 'mit Abdecken,' *i. e.*, by covering the drawings in part, or several times. So, in using this as a memory test, I have left the age level the same, though probably this will have to be modified, placed somewhat higher.

Ziehen's Test-Figure.

Color memory, (*c*), is tested (I) '*from memory*': Color of a one and two cent stamp,[3] strawberry, lemon, grass, sky, potato, coal, corn-flower, leaf, moon, warship, American flag, etc. For the second test, (II), *recognizing* colors, one shows several colored skeins of yarn, or strips of paper (for ten seconds), and then covers them over; after a few minutes a larger assortment is placed before the child, who is asked to

[1] One must not *tell* the child to 'draw a square'—one *shows* the square (1 to 2 inches in size) and then instructs the child to copy it. Binet gives pen and ink, this being more difficult than drawing with lead pencil. In drawing the diamond one half of his children failed at six, even one-fifth failed at seven. It would be interesting to know if any real differences occur, the child using pencil, pen or brush and paint.

Bobertag's statistics (testing with the diamond) are: at the age of six, 10 passed out of 31 (32 per cent.); at the age of seven, 27 out of 44 (61 per cent.). He also asks the child what it thinks of its own drawing. This, however, is *criticism*, and will be dealt with under 'Combination.'

[2] Treves ed Saffiotti, 'La scala metrica dell' intelligenza di Binet e Simon,' etc., Milan, 1911.

[3] Ziehen points out that mistakes between green and blue in the matter of stamps 'from memory,' are numerous in normal persons.

pick out the colors already shown. (Should be done at four to six. See also under Knowledge.) The *matching* of colors may also be employed in the same way. Test III. is of *naming* colors (cf. Warburg, *l. c.*). Binet shows four squares of colored paper (red, yellow, green, blue) pasted on a cardboard and has the child name them—"the verbalization of the colors perceived." He asks "What color is this?" pointing to one after the other. No errors whatsoever may be made, no reconsidering, etc. This must be passed at seven. Bobertag, as already stated, found that but one half his children at seven passed and therefore puts the test in the eighth year.[1]

Under (*d*), sentence repetition, any simple sentences may be given. (Binet's do not appear to be very well chosen.) One may make up sentences, giving several and increasing the length, having the child repeat these. One then knows about how long a line a child easily holds, and counts the syllables. Or one may write out a series of sentences in which the number of syllables is known, and use them as constant tests. Just below this test one will again notice, in brackets, how many syllables a child is supposed to remember at a given age. These sentences are to be repeated without an error, no word to be omitted, nor other substituted. The following are examples of sentences: "My brother went away" (6 syllables); "We have not finished our lessons" (8 syllables); "I am going to my mother to-day" (10 syllables); "Let us all go for a long walk this afternoon" (12 syllables—suggested by Bobertag, and considerably better than Binet's[2]). The last sentence may be lengthened to 16 syllables, thus: "Let us all go for a real long walk through Central Park to-morrow," etc.

[1] Meumann, on the other hand, asks for the naming of six fundamental colors (Hauptfarben) in the fifth year under his environment-tests; in the seventh year, for seven chief colors and three nuances (also the colors of the 5, 10, 20, and 40 pf. stamps, and the states' and country's colors); in the eighth year, for ten colors, also tests with exposing and covering of colors, and naming from memory similarly colored things. See his 'Vorlesungen,' etc., schema, at end of Vol. II.

[2] "I am cold and hungry" (6 syllables); "My name is Gaston—Oh, the naughty dog" (10 syllables); "Let us go for a long walk—Give me the pretty little bonnet" (16 syllables—Binet's—from Miss Towne's translation).

Under (*e*) Binet's paragraph may be used, or Goddard's. In this test the child reads the paragraph aloud. *It is a questionable test*, much depending upon the child's ability to read, whether its attention holds, or is distracted, how dramatic the paragraph is, etc. Its evaluation, finally, is extremely difficult.

Under (*f*), story remembrance, a short story, fable[1] or definition may be given. I have found the following often useful. I give the child a definition of an optimist (something that it probably never heard before): "An optimist," I say, "is a person who is always happy, always cheerful, always smiling, even when it rains or things go wrong. He always sees the bright side of everything." I have the child (*i*) repeat what I have said, noting (*ii*) the exactness of reproduction, and see to it that he has understood. I also have him promise to be an optimist. (*iii*) The question is again put on the following visit. Such a definition (because of the abstract involved),[2] as also the Binet and Goddard paragraph (because of the many incidental facts given), are much more difficult to repeat and remember than a fable or complete story. A good story, however, is used in the tests under *combination* further on, and it is well to compare the outcome of this test with that under combination.

Under (*g*) we test very recent, recent and more remote memory. We have the child tell what it did yesterday throughout the day, or what it had for dinner,[3] also what it did last Sunday. We likewise ask where it lived last year, went to school, etc. (Or one may ask "Tell me what you have been doing and where you have been in the last two years?") In adults this test is often of great importance, and the questions must be extended, and immediate memory also tested, *i. e.*, "Where were you just before coming here?"; "How did you come in?"; "What question did I just ask

[1] Terman and Childs give a fable, and find that it is repeated at ten. Meumann also uses this (fable) test.

[2] Here memory alone, and not comprehension, is tested. However, memory is always much aided through comprehension.

[3] But it should be remembered that even normal adults very frequently do not remember this.

you?" etc. We can also enquire about certain books read, or some special book, and what is remembered of the story. Or we learn of certain happenings in the family, and question the patient concerning them.

The carrying out of commissions (*h*) is Binet's test (puts keys on chair; shuts door; brings box). Bobertag holds this as almost wholly a memory test, though attention plays a part, and likewise thinks it somewhat too easy for the sixth year.

When doubtful as to the results of our Memory examination, or if a fuller examination is desired, other tests, such as Ziehen's *pair-words* method may also be tried; *i. e.*, one gives ten pairs of words, each pair composed of a noun and an adjective. After a period one gives the noun of each group, and asks for the adjective. (See *Monat. f. Psych. u. Neurol.*, Bd. 9, 1901; also 'Psychiatrie,' 3d edit., p. 229.) Ranschburg uses a similar method, giving street names and numbers. For other methods of examination, especially with apparatus, see second half of P. Ranschburg's 'Das kranke Gedächtniss,' Leipzig, 1911. On memory examination, etc., in pathological states, see A. Gregor's 'Leitfaden der Experimentellen Psychopathologie,' Berlin, 1910, 8th lecture. See also M. Offner's 'Das Gedächtniss, 'Berlin, 1911, especially Chapters VIII. and IX.

Let us pause here to take a look at the *bookkeeping* entailed so far:

3. MEMORY:	normal
(a) I. I go to the store and buy......(4 to 12 facts). II. 138, 692; 5734296 G III. Recalls (I). ✓ (2 or 3 *at 3;* 3 *at 4;* 4 *at 5;* 5 to 6 *at 6–7;* 6 to 7 *at 8;* 6 to 9 *up to 12.*)	E. very good.
(b) Optic memory: I. *f.m.* ✓ II. *at 5;* *at 6;* *at 7;* *at*	due to poor vision?
(c) Color " I. *f.m.* ✓ II. *at* 6. III. naming, *at 7–*	
(d) Repeats sentences: ✓ 30 syl. (6 syl. *at 3;* 10 *at 5:* 14 to 26 *at 6:* 16 to 26 *at 7–8;* to 30 *at 9;* 26 to 40 *at 12.*)	G
[(e) Remembers from paragraph read: (2 facts *at 8;* 6 *at 9;* 7 *at 12.*)] 8 facts.	E
(f) Story: I. repeats ✓ II. exactness F III. following visit G.	G
(g) Of yesterday: ✓ Last Sunday: ✓ Past: ✓ Immediate: ✓	✓
(h) Carries out commissions (3 *at 6*)......6	E

The case from which the above is taken was a child of ten years with corneal opacities, the right eye having such opacity near the center. The clinical diagnosis was that of congenital syphilis, with a question mark. Physically the child was normal; the senses were also normal, save for the lack of vision in the right eye. This child was almost in

the proper grade for her age. The memory examination here shows remembering and recalling to be excellent (the '7' inside the circle signifies seven digits repeated): recalling of forms (from memory) normal; but copying of the diamond was but fairly, if not rather poorly, done. This copying went slowly, and was accomplished with much effort. The first color test was normally passed, the second very poorly. It was evident, from the way the girl went about it, that her poor vision accounted for this. Hence, in the margin, we noted the fact. The sentence repetition (test *d*) was good, the remaining tests (save the second, under *f*) were good or excellent. Hence, in the right-hand margin we wrote at the top "Normal memory," also the comment under this "Very good." Here, at a glance, we can see what was good, and what was not good in the child's memory. The note as to the defective vision explains the poorness of some of these tests.

All in all, our memory tests embrace both the ability to take up or 'take in' new impressions (*i. e.*, momentary *impressibility* of the mind for new matter—the German Merkfähigkeit, corresponding to our 'rote memory'), as well as the faculty to retain the matter absorbed. Children are astonishingly capable in both—hence meager capability makes us suspect a defective mind.

There is great variability, however, in normal individuals as to their endowment in these faculties. Some take up new ideas rapidly, but forget them rapidly. Others learn slowly, or with difficulty, but the matter 'sticks' and is remembered. Others again, even though they learn with difficulty, nevertheless forget easily.

The taking up of ideas is influenced greatly by attention and interest. When we tire attention fags, and things taken in won't 'stick'—the impression doesn't hold. Hence, where there is tiredness and fatigue (when the brain is anæmic the same condition obtains) or lack of attention or inability to attend (because of lack of interest, poor teacher, or pain, hunger, etc.) things are not learned, *i. e.*, are not taken up nor retained. This is very important to remember. But still more important is the fact that the abnormal child, especially the debile or light imbecile, has a greater fatigue-ability than the normal, and that the results of an examination may be very different according to whether the child is fresh and rested, or physically and mentally tired.[1]

Another point of interest (possibly of great importance) is that some studies have shown that during periods of rapid growth the intellectual functions are weaker, that the energy for growth is drawn upon at the expense of the energy used in the intellectual development.[2] This probably involves all the intellectual faculties, not

[1] O. Binswange rhas called especial attention to this. See 'Jahreskurse f. Aerztl. Fortbildung,' 1912 (3 Jahrgang).

[2] See Claparède's 'Studies in Child Psychology and Experimental Pedagogy,' Chapter 4, No. 2, and other literature cited by him.

memory alone. The ages between six and seven, and fourteen and fifteen are the periods especially affected.[1]

The more ideas in the brain to which to bind the new incoming material, *i. e.*, with which to associate it, the more easily the new matter is remembered. If there is nothing to associate it with the difficulty of remembering is great. This may be tested by giving senseless syllables to be retained or words from a foreign, unknown language. (Great poets, with astonishing literary memories have been abominable spellers. Spelling is learned much as the senseless syllables just referred to, and becomes almost wholly automatic, while literary knowledge is rich in associations.)

That which is associated with emotion[2] (directly accountable for the emotion or associated with it) makes the deeper impression and is retained longer. This is also true of *interest*. The longer the perception lasts, the stronger the stimulus is, or the oftener it is repeated, the profounder is the impression and the more durable the memory. Intensity and extensity (because of newness, bizarreness, largeness, the exceptional, terrible, painful, etc.) influence the durability of memory.

Observation and experiment have also shown that if the thing studied or learned is followed by a pause or rest period, it is retained longer[3] (the retention 'mellows' as it were), while if something directly follows, the retention of the first is weakened.

Again, memory is really composed of many special memories. Some people may have good memories for certain things (names, faces, dates, words, colors, etc.) and very poor ones for other things. In the latter case, the condition may even equal a 'defect.' This must also be borne in mind in examining children. Debile or imbecile children may show the same characteristics—that is, greater ability for certain types of memory, and less ability for other types. Hence, the tests must cover the various types of memory. Only recently Ziehen called attention to this 'partial intelligence defect,' which he says has not been sufficiently noted.[4]

There are still other types of memories characterizing individuals, and which should be thought of, namely, the motor, visual and auditory.

(To be continued.)

[1] Contrary fluctuations within the year (as to seasons) have also been noticed. Thus Schuyter in Antwerp (cited on p. 5 of P. R. Radosavljevich's 'Die Entwicklung des Kindes innerhalb der Schuljahre') found that the months in which the weight increase is smallest are those in which the intellectual growth is least. March, April and May appear to be the months (according to Malling-Hanson) in which the weight and growth are least.

[2] Netschajeff notes (cited by Gaupp) that affectivity in association with words and ideas appears to play a relatively small part between the ages of 9 and 11, but grows rapidly in importance in puberty. In his tests, however, only Russian children were studied.

[3] See R. Gaupp's 'Psychologie des Kindes,' Leipzig, 1910, p. 95. Rest pauses have likewise a very great influence on attention.

[4] Beitrag zur Beurteilung des sogenannten 'Moral Insanity,' etc. *Der Praktische Arzt*, No. 19–20, Leipzig, 1915.

A SUBSTITUTE FOR AN ARTIFICIAL PUPIL

BY C. E. FERREE AND GERTRUDE RAND

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It is a well-known principle of physiological optics that the amount of light condensed into the retinal image of a given test object or source of light varies with the size of the pupil. It is obvious, therefore, that a complete plan or provision for the standardization of the factors which influence the precision of a color determination must take into account the possible sources of error due to a variable pupillary aperture.[1] The remedy usually employed when any account is taken of the factor is an artificial pupil. Whether, however, the use of an artificial pupil is to be recommended is seriously open to question. In our own work we have found its use to be attended by so many difficulties and to be open to so many objections as to be quite inadvisable, unless perhaps in case it is wanted to compare the response to stimuli differing rather widely in intensity. Space can not be taken here for a detailed discussion of these difficulties and objections. It will be sufficient perhaps to say that with the best possible adaptation of the size of the artificial to the natural pupil, its distance from the eye, etc., four difficulties remain which seem inherent and very difficult if not impossible to overcome. (1) The influence of the brightness of the surrounding field on the sensitivity of the retina to the test field can not be satisfactorily controlled.

[1] We will say, however, that we believe this variable error to be very small when a constant light flux is used for stimulus, and a light-adapted eye is employed, working at a moderately high and constant intensity of illumination. We believe this because of the very small mean variations we have obtained in determinations of color sensitivity when all the other factors influencing the response of the eye, which we have discussed in previous papers, are controlled and the pupil is left to regulate itself. In fact we have not been able so far to reduce appreciably the size of this mean error by any artificial regulation of the constancy of the light flux entering the eye from a given constant source. Regulation is desirable, however, when the effect on the eye of different intensities of light is to be compared.

Theoretically considered it would not be possible to attain this control unless the artificial diaphragm could be brought approximately into the plane of the iris. (2) The response of the retina can not be investigated out to the peripheral limits of the field of vision. With the intensity of light attainable with the apparatus described in a preceding paper,[1] red, blue, and yellow can be sensed out to 92° and green to 70° in the nasal meridian. (3) The relation of size of pupil to the cross section of the beam of light which the artificial pupil admits to the eye can not be under observation while the color determination is being made to see whether the regulation needed is actually accomplished in any given case. The adaptation of the size of the artificial to the natural pupil must rest upon a probability established by a number of measurements made on the reaction of the pupil under a set of conditions as nearly as possible identical with those used in the case in question. And (4) the device used to give the pupillary aperture can not be gotten so close to the natural pupil as not to disturb the adjustment of the eye and otherwise to serve as an annoying distraction in the field of vision.

In the apparatus described in the preceding paper the method of presenting the stimulus to the eye is such as to permit of a substitute for the artificial pupil which so far as we are able to determine is entirely free from the objections and difficulties attending the use of a diaphragm in front of the iris. In this substitute plan instead of cutting down the cross section of the beam of light at the eye by means of an apparatus which interferes with the natural functioning of that organ, the regulation needed is accomplished further back in the optical system, out of range of the anterior reactions of the eye and out of the road of the manipulation needed to control the factors which directly influence the response of the eye to its stimulus. That is, the stimulus light is focused by means of the lens (L_2)[2] upon the pupil of

[1] Ferree and Rand, 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral,' *J. of Exp. Psychol.*, 1916, 1, 247–284.

[2] *Op. cit.*, pp. 254–255.

the eye, forming an image of the analyzing slit of the spectroscope. Obviously the size of this image can be regulated by controlling the height of the slit, for example, or by means of the lens system so as always to fall within the aperture of the pupil reacting to the intensity of the light used in any given case. It adds very much to the precision of the regulation also that its correctness can be checked up if desired for every color determination while the determination itself is being made.[1] That is, not only can the size of image needed for a given intensity of light and set of conditions be determined empirically in a number of trials but its relation to the size of the pupil may be under observation all of the time in a series of determinations of sensitivity. With the lens system and breadth of slit we are now using, we have found that it is not necessary to alter the breadth of the image. This would be done, if it were necessary, by means of an alteration in the lens system. The height is reduced the desired amount by cutting down the height of the analyzing slit which was made variable over a wide range primarily for this purpose, as was stated on pp. 253 and 261 of the preceding article: 'A Spectroscopic Apparatus for the Investigation of the Color Sensitivity of the Retina, Central and Peripheral.' This control of the constancy of the amount of light entering the eye we have found to be entirely feasible and practicable. With it in fact the observation is attended with no more difficulty than if the apparatus were used with no attempt to exercise this control.

[1] Owing to the brightness and sharpness of the image on the cornea with its dark background of iris and pupil, this comparison of the size of the image with the size of the pupil is, it is obvious, not difficult to make. The possibility of having this feature constantly under observation gives this method of regulating the amount of light entering the eye no small advantage over the artificial pupil.

PORTABLE TACHISTOSCOPE AND MEMORY APPARATUS[1]

BY W. F. DEARBORN AND H. S. LANGFELD
Harvard University

The front of the tachistoscope is a brass surface $9\frac{1}{4} \times 4\frac{1}{8}$ inches with a window $2\frac{1}{4} \times \frac{3}{8}$ inches. The window is closed by a paper shutter, which is worked by a brass spring. The speed is below $\frac{1}{10}$ of a second, that is, faster than eye-movements and can be easily regulated by tightening the spring. The shutter mechanism is behind the brass front and entirely concealed from the subject. By pressing the button at *A* the subject can operate the shutter himself or the experimenter can operate it by a string from behind. The brass front is on hinges and can be opened to insert the list of words. A small number is placed to the right of each word in the list and this number can be seen through the small hole *B* at the right of the shutter. It is thus known when the word is behind the shutter and what it is. By leaving sufficient space between the words and indicating the space position by dots on the margin (see sample list, p. 386) the shutter can be closed without exposing the word.

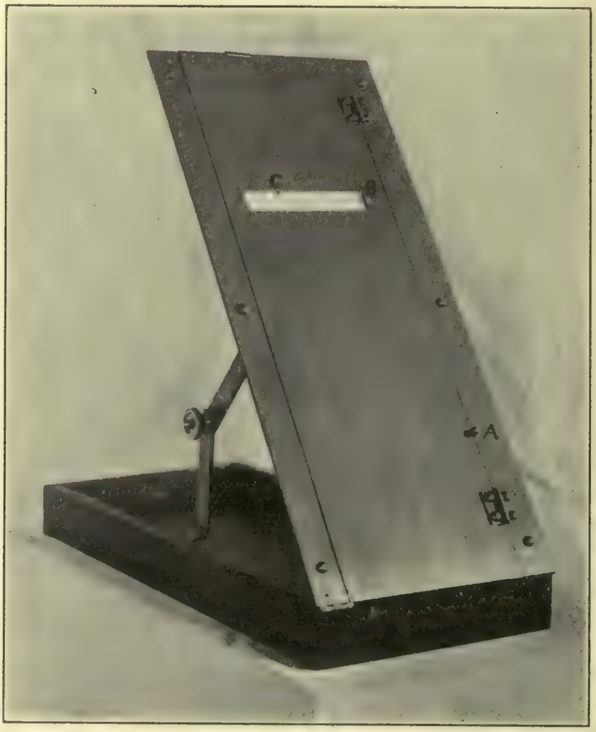
When the window is closed the paper shutter, by sliding between the brass front and a metal contact, opens an electric circuit. The contact is made when the shutter falls and the word is exposed. The exact time of the contact can be regulated by altering the height of the paper shutter.

By half cocking the shutter the window can be kept open. The angle of the brass front to the base can be altered to suit the convenience of the subject and the illumination. When the instrument is not in use the front can be closed, and it then fits into the base so that one has a closed box only an inch in thickness, which can easily be carried in the pocket. The instrument weights 17 oz.

[1] The instrument can be obtained from A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

There is an adjustable brass pointer at *C* which may be used for the purpose of fixation and to direct the attention to the beginning, ending, or various parts of the words.

The instrument meets most of the requirements of an ideal tachistoscope: (1) There is no appreciable change of accommodation, as the point of initial fixation is separated from the word only by the thickness of a thin library card



Portable Tachistoscope and Memory Apparatus.

(paper shutter), a negligible quantity from the ordinary reading distance. (2) The tendency of the eye to follow the moving shutter, which is the disadvantage in most gravity tachistoscopes, is minimized in that the shutter and background are of the same color, and this tendency may be further decreased by enamelling the front surface of the instrument the same shade as the shutter. If this is done the

conditions of ordinary reading of black print on a white surface are more nearly duplicated. (3) The movement of the shutter is sufficiently noiseless not to be disturbing. (4) By reducing the instrument to a very simple form of construction it has been made sufficiently inexpensive to allow its use in quantities in training courses. (5) There is an advantage in the observer being able to release the shutter, as he can thus concentrate his attention on the window at the right moment.

The following are a few suggestions for experimentation, especially in training-courses:

1. As indicated in the sample list, the visual span for objects, numbers, letters, familiar words of various lengths, unfamiliar words, and short sentences may be investigated.

2. Observations may be made in regard to the reading by whole words or parts of words. For this purpose in addition to the unfamiliar words, mutilated words such as 'phylosophical,' and skeleton words such as 'ab-r-vi-t-d' may be used. An example of the reading by parts is as follows:

1st reading	....	Cyt
2d "	....	Cytore
3d "	....	l ticum
4th "	....	Cytorel
5th "	....	Cytore
6th "	....	culum
7th "	....	ticulum
8th "	....	reticulum
9th "	....	Cyto
10th "	....	Cytoreticulum

3. In order to investigate differences in the form of words, lists of words composed mainly of words with characteristic or determining letters or letter complexes and others composed of the more indifferent letters such as the letters of the line may be used. 'Alphabetical' and 'statistical' are examples of the former, 'conscience' and 'caravan' of the latter.

4. The suggested classification of readers into objective and subjective types may be tested, *e. g.*, by lists of words easily misread. The objective type is supposed to include

readers who have a narrow span but accurate perception, the subjective a wider span but a more suggestive perception. The subjective are supposed to depend in their perceptions more upon the total visual form, the objective more upon the determining or dominant letters.

5. As a test of range of information lists of words from various fields of knowledge may be used.

6. In order to test the influence of a definite mental set two lists may be used, the one composed of relatively unrelated words, the other of words from a specia field.

7. For memory experiments the shutter, as mentioned above, is placed at half cock, which leaves the window open. The lists of words are drawn past the window at a rate regulated by a metronome or sounder in circuit with a clock. The back of the instrument is so constructed that a belt of paper may be used instead of strips, thus obviating loss of time between the successive exposures of the list.

8. The electric contact permits the instrument to be used in reaction experiments by placing the contact in series with battery, chronoscope, and voice key.

N. B. If the instrument is manipulated entirely by the observer, lists may be placed in the instrument with a blank sheet over them and the blank sheet slipped out before starting. This is a useful method in case small classes of pupils in schools are to be tested.

Sample List

****	1.
*****	2.
******	3.
*******	4.
8197	5.
68352	6.
827153	7.
1852379	8.

bdcv	9
zjflmt	10
trpbhcd	11
kvnsqgtc	12
watch	13
books	14
wires	15
maple	16
run away	17
how do you do	18
practical	19
multiplication	20
statistical	21
alphabetically	22
caravan	23
environment	24
loquaciousness	25
micromaniac	26
succinamic	27
expansile	28

PORTABLE SELF-REGISTERING TAPPING-BOARD AND COUNTER[1]

BY HERBERT SIDNEY LANGFELD

Harvard University

The instrument consists of a small tapping-board with a brass tapping surface 5 × 3 inches (*A*), a stylus (*B*), and a box $5\frac{1}{2} \times 4\frac{1}{2} \times 3\frac{3}{4}$ inches (*C*) containing a Veeder counter, magnet, and a five-cell Eveready battery. The board is hinged to the box and can be folded up and hooked (at *D*) to the front of the latter, thus making the instrument very

compact, and convenient for carrying. The board is connected to one of the poles of the magnet through the hinge, and the stylus to the other pole. The magnet, which works the counter by means of a curved armature fastened directly to the shaft of the counter, is quick in action and the fastest

[1] The instrument can be obtained from Mr. A. G. Cox, the mechanic of the Harvard psychological laboratory, Emerson Hall, Cambridge, Mass.

tapping can be recorded accurately. The number of taps are read off on the Veeder through a small window in the top of the box at *E*. With this instrument, tests for muscular coördination and fatigue can readily be made in the schools and factories as well as in the laboratory and the results recorded with a minimum expenditure of the experimenter's time.

By substituting a finger ring for the stylus the instrument can be used in finger movement experiments similar to those described by the writer.[1] By a simple adjustment it can be employed to record the free finger movements recommended by Professor Raymond Dodge.[2]

Instead of the tapping stylus a contact pencil (*F*) can be connected to the magnet. This has a metal point (*G*) which, when slightly pressed down, closes a contact in the pencil and actuates the counter. With this pencil, curves, words of a manuscript, etc., can be counted accurately and without mental fatigue.

[1] 'Facilitation and Inhibition of Motor Impulses,' THE PSYCHOLOGICAL REVIEW, Nov., 1915, 22, 6.

[2] 'Psychological Effects of Alcohol.' Washington: Carnegie Institution of Washington, 1915, p. 167.

A NOTE ON FERREE AND RAND'S METHOD OF PHOTOMETRY

BY H. M. JOHNSON

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In a recent communication[1] Ferree and Rand describe a unique method of photometry, which has been in almost daily use in the Bryn Mawr laboratory for four years. In this method, the compared portions of the visual field, instead of being brought as nearly as possible into juxtaposition, have an angular separation of 20° to 25° at the eye. ne of these elements is regarded directly and the other obliquely. The comparison sought is between the amounts of contrast exerted on the compared elements by a campimeter screen lying between them and filling a good part of the visual field. This screen has a higher reflection-coefficient than the compared elements and is illuminated by the same source as are the parts compared. The relatively greater sensitivity of the peripheral retina to the effect of contrast is compensated for by reducing the reflection-coefficient of the compared element which is regarded directly, until the two elements appear to match. The amount of this reduction is taken as an index of the illumination on the campimeter screen. When this appearance of the compared elements under a standard lamp at a known distance is reproduced under a second source substituted for the standard without any change in the reflection-coefficient of the variable element, the illumination on the screen is assumed to be equal under the two conditions; and the luminous intensity of the second source is regarded as computable from its distance from the screen.

The authors propose two conditions to be fulfilled by any satisfactory method of photometry, and submit their method

[1] Ferree, C. E., and Rand, Gertrude: 'A New Method of Heterochromatic Photometry,' Journal of Experimental Psychology, 1916, 1, 1–12.

to test in accordance with them. (1) The method should be sensitive, and its results reproducible; and (2) "it should be known either to possess of itself logical sureness of principle, or its results must agree in the average with those of some method which can be shown to possess this sureness of principle." They claim for their method (1) that with respect to both sensitivity and reproducibility it surpasses the method of direct comparison, or "equality of brightness method," even when the photometer used in the latter method is of the best Lummer-Brodhun type. Applying their criterion (2) they do not discuss the question of whether the method is sound in principle, but they do claim in its favor that the results obtained by it agree very closely in the averages with those obtained by the method of direct comparison, and have an advantage over the results obtained by the latter method in that they are more consistent.

The object of the present discussion is to point out that these claims are not supported by the data which the authors present as proof. For the sake of brevity I shall refer by pages to the original article instead of quoting from it; and the remarks below should be read with the original article at hand for verification.

The authors do not go into details in several important particulars, one being the linear separation of the compared portions of the visual field. However, from the distance given between the eye and the campimeter screen (p. 6) and the angular separation of the compared portions at the eye (p. 4), one concludes that the compared elements were about 10 cm. apart. The vertical plane of the optical bench (photometer bar) was midway between the compared elements (p. 5), and the authors assumed that these elements "received equal amounts of light from the source to be measured." Even if the elements were equidistant from the lamp—as happens not to have been the case (p. 4)—the truth of this assumption does not follow from the data given. In some of the work the results of which are presented in the authors' table (p. 9), the angular separation of the compared elements was 14° to 15° at the source. Now the radiation

from a carbon or tungsten lamp is not equal in all directions, as is that from an ideal point source. In fact, for lamps of such types differences of several per cent. in different directions normal to the long axis of the lamp are the rule, and a considerable difference might occur in a range of 15°. For this reason it is customary for standardizing bureaus to scratch fiducial marks on the bulbs of the lamps which they send out, to enable the experimenter to work in the direction for which the luminous intensity of the lamp has been determined. True, it would have been possible to overcome this difficulty by using as a standard lamp one whose average intensity in different directions in the horizontal plane was known, and by placing the compared lamps in a rotator, so as to utilize their average intensities. As the authors do not mention such a procedure and do not show a rotator in their figure, one would be justified in assuming that the precaution was not taken. It may be well to mention in passing that the likelihood of error from this source would be increased by adopting the authors' recommendation (p. 7, footnote) that the observer be seated farther from the campimeter screen, and the compared elements given a correspondingly greater linear separation.

Due to lack of information from the authors it is impossible to get at the magnitude of these errors, although the probability is that they were not very large, especially with respect to errors from another source which I shall presently indicate. We may therefore drop the former from present consideration.[1]

It should be noted that the plane of the "measuring disc"—a part of which constituted the compared surface

[1] Another source of error which the authors appear not to have taken into account may be worthy of mention. The angles at which the light was diffusely reflected into the eye from the stimulus-patch and the disc at the fixation point were not the same. The percentage of incident light reflected into the eye would have been different, therefore, even if the two surfaces had been of the same material. Furthermore, the difference in percentage of incident light reflected in the direction of the eye is not constant for any two positions of the source. Cf. Wright, H. R.: 'Photometry of the Diffuse Reflection of Light on Matt Surfaces,' *Philosophical Magazine, etc.*, series V, **49**, pp. 199–216. The magnitude of this factor also cannot be estimated in the present case from the data given, but it is probably overshadowed by the other errors.

regarded directly—stood 3 cm. nearer the lamp than the plane of the part of the field with which it was compared (p. 4). The reason for this is not clear. The authors say that it was 'primarily to eliminate any induction by the screen on the disc.' It is not immediately evident how the effect of contrast could be eliminated by placing the test-field and its immediately visible surroundings in slightly different planes; nor is one readily convinced by the evidence which the authors present as showing that this effect was practically eliminated. In fact, it is not even clear why the authors considered it necessary to eliminate it, since the method depends on the difference in sensitivity of the central and peripheral regions of the retina to the effect of contrast. Be that as it may, this disposition of the parts certainly vitiates the assurance given by the authors (p. 5), that 'the contrast-stimulus and the measuring disc thus receive equal amounts of light from the source to be measured.' Neglecting the departure from point-source conditions mentioned above, let us note the effect of this difference in distances on the results as presented in the authors' table (p. 9).

The data presented in the third column give the 'distance of white light [source] giving equality of illumination' as compared with certain lamps at given distances with colored filters interposed, as determined by the authors' new method and by the method of direct comparison ('equality of brightness method'). The authors do not describe their mode of procedure in making their measurements by the method of direct comparison. I assume, therefore, that they used the standard procedure, in which the 'test-lamp' and the standard lamp are successively placed on one side of the photometer head, a third lamp being kept at a fixed distance on the opposite side; the whole system being mounted on an optical bench, and suitable protective diaphragms being interposed to exclude extraneous light. Under these conditions, and working with the lamps beyond certain minimal distances from the photometer-head, the luminous intensities of the compared sources would be inversely as the squares of their distances from the photometer screen at valid settings for

equality of brightness on the two halves of the photometer field. Assuming that this set of measurements was made in this way, let us examine a little more closely into the comparison between these results and those obtained by the new method, according to the authors' table.

It appears from these data that the average settings by the new method are very consistent, and that they agree within a fraction of one per cent. with the average settings made by the method of direct comparison, in which the inverse square relation may be supposed to hold. This agreement is hard to understand, in the light of facts derived from the authors' own data, and presented in the accompanying table.

Source	Filter	Colored Illumination			Colorless Illumination Giving Same Appearance of "Contrast Stimulus" and "Measuring Disc"			Deviation from Proportionality of Illumination, Per Cent.
		Distance of Source from Plane of Screen	Distance of Source from Plane of Disc	Illumination on Disc Behind Fixation-Point, Per Cent. Illumination on Screen at Stimulus Opening[1]	Distance of Source from Plane of Screen	Distance of Source from Plane of Disc	Illumination on Disc Behind Fixation-Point, Per Cent. Illumination on Screen at Stimulus Opening[1]	
87 c.p.	Red	41 cm.	38 cm.	116.1	66.6 cm.	63.6 cm.	109.6	6.5
" "	Blue-green	41	38	116.1	59.5	56.5	110.7	5.4
52 "	Red	38	35	117.5	82.2	79.2	107.4	10.1
" "	Blue-green	38	35	117.5	70.5	67.5	109.0	8.5
13 "	Red	38	35	117.5	160.0	157.0	102.0	15.5
" "	Blue-green	38	35	117.5	134.9	131.9	104.5	13.0

In brief: by the authors' method, the stimulus-patch and the measuring disc were not equidistant from the lamp, and consequently received unequal illumination from it. The difference in illumination being approximately the difference between the reciprocals of the squares of the distances of the illuminated surfaces from the lamp, it was not proportional for any two positions of the lamp on the photometer bar. When the colored sources were used, the compared elements

[1] Assuming applicability of inverse square law, and taking as distance between source and illuminated point the hypotenuse of a right triangle of which one leg is 5 cm. (the distance of the stimulus opening and the fixation point from the axis of the optical bench) and the other leg the distance of the source from the plane of the screen, or disc, as the case may be.

appeared to match when the difference in illumination was 16 per cent. to 17.5 per cent. in favor of the disc. When a colorless source was used instead of the colored source, and placed at the proper distance to give the same illumination on the screen as the latter received from the colored source,[1] the patch and the disc presented a satisfactory match when the reflection-coefficient of the disc was the same as under the colored lamp. If the method were valid, we should expect a match to hold only when the same proportionate difference in the illumination on the patch and on the screen existed as in the former case. Nevertheless, instead of the difference of 16 per cent. to 17.5 per cent. which obtained at a match under the colored source, the difference under the colorless source was only 2 per cent. to 10.7 per cent. The discrepancy, as shown in the last column of my table, varies between 5.4 per cent. and 15.5 per cent. Thus the agreement in settings made by the authors' method and by the method of direct comparison would appear to be spurious evidence of agreement between the two methods, and good evidence that the authors' procedure in making the settings was faulty.

The authors give no information which indicates where the trouble lies, nor do they appear to have noted the discrepancy. In an ordinary situation of like kind one would suspect that the settings made by one method were influenced by knowledge of the settings made by the other method. I do not wish to be understood as offering that as an hypothesis in the present case, however; nor do I wish to divert the discussion from the main issue by proposing any other hypothesis.

To conclude: the data presented by the authors, taken at face value, indicate that the method is insensitive, since large deviations from proportionality of difference in illumination on the compared portions of the field were not detected by it. While the authors' data indicate that the settings made by their methods are highly reproducible, the thing reproduced is an error the magnitude of which is large, and

[1] As determined by the method of direct comparison, in which the inverse square relation may be assumed.

not even a function of the illumination received from the compared sources. The authors do not claim soundness of principle for their method, and their claim that its results agree with those obtained by a sound method is seen to be erroneous.

Unpromising as the method may appear in principle when considered *a priori*, it would be unscientific to condemn it without a fair and thorough trial, which it apparently has not yet received at the hands of its originators.

EXTRAVAGANCES IN THE MOTOR THEORIES OF CONSCIOUSNESS

BY H. C. McCOMAS

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The biological point of view in psychology has brought the so-called motor theories of consciousness into sharp relief. As yet there has been no formulation of these theories into a coherent whole. They represent a tendency in the thinking of today and not a consensus of opinion. The disposition to interpret consciousness in motor terms is not new; it may be found in many of the earlier writers of modern psychology who sought explanations in the motor factors for various aspects of consciousness, such as attention, space-perception, ideo-motor actions, emotion, and volition. Today the more ambitious program would include all aspects of consciousness. Broadly stated, all consciousness is conditioned or accompanied by motor activity. Such assumptions may be detected in much of the recent work in psychology. It is difficult, however, to find completely rounded out theories. Of these the most influential are probably those of Dewey, Münsterberg, Judd, and Watson.

Chronologically Dewey[1] stands in the forefront. His conception of the reflex arc as converted into a circuit laid great stress on the motor side of processes that were commonly considered sensory. The sensory-motor processes are all one process in this scheme. "It is just as true to say that the sensation of sound arises from a motor response as that the running away is a response to the sound." The reason for this lies in the fact that "the sound is not a mere stimulus or mere sensation; it again is an act, that of hearing. The muscular response is involved in this as well as sensory stimulus. . . . The movement and posture of the head, the tension of the ear

[1] J. Dewey, 'The Reflex Arc Concept in Psychology,' PSYCHOL. REV., 1896, 3, 357-370.

muscles, are required for the reception of the sound. . . . The conscious sensation of sound depends upon the motor response having already taken place. . . . Indeed, the movement is only for the sake of determining the stimulus, of fixing what kind of stimulus it is, of interpreting it."

Dewey's concept holds sensory, central, and motor processes as coördinations of one process. This would certainly imply that the motor elements were indispensable in consciousness.

Münsterberg's[1] 'Action Theory' maintains that every sensation and every element of consciousness is dependent on the passage of stimulation into discharge in the cortex of the brain. The quality, intensity, and vividness of sensations depend upon the parts of the cortex affected and the character of the current passing through to the motor tracts. In a recent formulation of his theory[2] he declares that whether a color or a noise become vivid in the mind or are refused admittance depends upon the conditions which prevail in the motor centers. If the channels of discharge are closed the sensations will be inhibited. "According to the popular view a world of impressions and ideas exists in us, entirely independent of our actions; and when they are complete and perfect they send their message to some motor apparatus which carries out the order. Such a fancy must be entirely reversed. In every moment the motor situation decides the possibilities in the sensory sphere. Our ideas are the product of our readiness to act." Whether the author means ready to act or action is not clear, for in another place[3] he declares: "The centripital function appears to be almost more important than the centrifugal one which bears the stimulus. It is the action of the organism which controls the mental life." It would seem from the following that an actual movement were necessary: "The personality's thinking is as much the product of his actions as his actions are the product of his thought."

In Judd's psychology[4] the motor theory is everywhere

[1] H. Münsterberg, 'Grundzüge der Psychologie,' Bd. I., S. 548.

[2] 'Psychology, General and Applied,' 139–144.

[3] *Op. cit.*, 423.

[4] C. H. Judd, 'Psychology,' 71, 134, 296, 183.

present. In an early chapter he states, when analyzing consciousness: "In some form or other, every incoming sensory impulse and every central nervous process must issue in a motor discharge. It is equally true, though by no means so obvious, that every form of conscious experience is related to behavior." In discussing sensations he says: "Our problem is not exhausted, however, when we have dealt with the interrelation between sensations, for every sensation is related in some way to the active processes of expression." In the complex associational processes there may be a delay of minutes, or days, before the excitation is "discharged through the motor centers in the form of a reaction." His conviction that "there is a universal relation between consciousness and bodily activity," rests upon biological grounds. "The structure of the nervous system from the hydra to man is such that there is always a motor organ linked with every sensory organ."

With Watson[1] we come to the logical culmination of this trend of thinking. "The nervous system functions in complete arcs. An incoming impulse exerts its effect relatively immediately upon one system of effectors or another." So precise is this relation that, "in a system of psychology completely worked out, given the responses the stimuli can be predicted; given the stimuli the responses can be predicted." The responses may be 'explicit' or 'implicit'; in the latter case they involve chiefly the actions of speech mechanisms. If these could be detected and recorded the material which the introspectionist claims as peculiarly his own would be accessible to the behaviorist, and the thought processes would be interpretable in terms of their motor accompaniments. So confident is Watson in the principle of the complete arc functioning in thought and speech that he declares: "If we could find a case where a man suddenly lost his laryngeal apparatus without any serious injury to the bodily mechanisms, we should have a crucial case. From our point of view there would, or ought to be, a serious limitation in this man's thought processes."[2]

These brief citations do not do justice to the author's

[1] J. B. Watson, 'Behavior; an Introduction to Comparative Psychology,' Chap. I.
[2] *Ibid.*, 327.

theories in their entirety; that would be impossible in a short article. Nor is it the present purpose to discuss these theories. The issue in hand is to point out an obvious extravagance running through this trend of thinking; namely, the insistence that a motor expression accompanies *all* conscious processes. No one will deny that there is a deep-seated tendency for the incoming impressions to go out into motor expressions; but there is nothing more than a *tendency*. The authors quoted above, and a number of others who have written less fully and frankly, push this principle to an extreme. They are seeking to make it an open-sesame into the most intricate problems in psychology.

To put the most obvious criticism into logical form, the champions of the motor theories commit a fallacy in generalization. They infer of a whole class what we know only of a part.

Moreover, they are captivated by the principle of continuity. The structure and the function of the nervous system in its evolution do undoubtedly show the intimate relation of receptor and effector, of stimulus and response. It is very easy to assume that a principle which is corroborated so frequently in the animal scale should apply to the most intricate type of nervous system. But does it? Facts alone can answer the question.

The influence of biology is clearly discernible in these theories. The psychologist who thinks of the nervous system in terms of the hydra may readily imagine every stimulus effecting a prompt response, but one who thinks in terms of human anatomy can hardly do so, for the anatomist himself is too circumspect. Villiger,[1] for example, insists that "only after traversing numerous neurones does the impulse finally reach the motor center and from there pass to the motor path, since coöperation of the various cortical centers must be assumed in explanation of the complex psychic processes." If this passage were by simple nerve trunk conduction every stimulus might go out into a response, but it is not so. A series of obstinate neurones and synapses must be traversed in the cortex and an excitation may die out anywhere along the

[1] E. Villiger, 'Brain and Spinal Cord,' 133.

line. Sherrington[1] shows that the reflex chain of several neurones offers higher resistance than that of a few neurones. What would happen if an impulse stalled in some high-threshold synapse before reaching a motor tract? Nothing, says the motorist; it must cross the mystic line (Münsterberg); it must make the round trip (Watson). Only when it crosses the frontier and enters the motor tract is it given a passport into consciousness.

If this be so, then the motor areas are most important for consciousness. Injuries in them should do greater damage to consciousness than injuries elsewhere. Southard[2] finds the reverse to be true. Injuries in the posterior areas cause greater disturbances than those in the anterior. Cushing[3] found that stimulating sensory areas did give an effect in consciousness, but the same stimulation in motor areas did not. Even if we ignore the cortex and consider the structure of the afferent and efferent tracts we can find nothing to support the motorist's ideal. According to his schema the two sets of tracts have equal tasks, they are parts of one circuit, or process, the motor being as active and important as the sensory. Anatomically, nothing of the kind exists. The sensory tracts outnumber the motor by three or four to one, and there are more neurones in the former than the latter. Moreover, when we compare the complexity of the sense organs with the simplicity of the muscles and glands the disparity is overwhelming. Washburn's[4] contention that "every sensation that can be discriminated in a fusion, and every group of sensations that can be attended as a single whole has connected with it one or more movements which are peculiar to it alone," calls for a much more elaborate motor apparatus than nature supplies. It is inconceivable that each fiber in the basilar membrane, affording over twenty thousand discriminable tones, should have its accompanying little movement with a meaning all its own. Indeed, if any inference is to be drawn from the structure of the human animal it must give a greater prominence and

[1] C. S. Sherrington, 'The Integrative Action of the Nervous System,' p. 156.

[2] E. E. Southard, Psychol. Bull., 1914, 11, 66.

[3] H. Cushing, *Brain*, 1909, 32, 44–53.

[4] 'The Function of Incipient Motor Processes,' Psychol. Rev., 1914, 21, 376–390.

importance to the sensory organs, tracts, and areas than to the motor. The two systems are not on a parity. Nothing but the exigencies of a theory could assign them equal duties in conscious processes.

Nor are the motor theories in better case when we turn to experimental results for evidence of invariable response to stimuli. Truly, many organic changes accompany various conscious states, but a close correspondence of the one to the other is far from proved. Shepard[1] in summarizing the results of work in this field says: "No factor, vasomotor, rate or amplitude of pulse, position or emphasis of dicrotic, or rate or amplitude of breathing, changes one way for agreeable and the opposite way for disagreeable conditions. . . . The rate of the heart is sometimes increased on the average, sometimes decreased, sometimes not changed at all. . . . The rate of the heart with sensory stimuli increases about as often as it decreases. Mental application gave a slight amount of vasomotor change, frequently so slight as to be hardly noticeable. . . . In about half of the mental application tests there are both vasodilation and vasoconstriction in a single test. When the change is only in one direction it is an even chance that it be dilation or constriction." Surely, no one would venture to predict the stimuli, given the responses, in that mêlée of confusions; and only the hardiest of theorists would seek to demonstrate how the organic movements determine the conscious states from that material.

The experimental results bearing upon the relation of unconscious movements to impressions and ideas are also inadequate to the motorist requirements. Stein[2] found that it was possible to educate many subjects to make automatic movements. However, there was a large percentage of both males and females from whom she could get no automatic movements at all. The so-called ideomotor reactions are nothing more than habits. They do not rest upon any physiological principle similar to that of reflexes, or instincts. They grow up out of experiences as do all associations. As we

[1] *Amer. J. of Psychol.*, 1906, 17, 522–584.

[2] Psychol. Rev., 1898, 5, 295–306.

habitually associate the words 'cat' and 'dog,' 'pretty' and 'girl,' so too do we associate an oncoming baseball and the catcher's movement, or the threatened blow and the pugilist's 'on guard.' Many of us accompany our language with gestures. These easily become habitual and accompany certain thoughts which may or may not be expressed in words. This conception of ideomotor action is supported by a study of the involuntary movements of children and adults.[1] Among children there is less directness in such movements than in adults. In imitating unconsciously the movement of a moving object they are less susceptible than adults. They show less fixity than adults in both their movement and verbal associations. It is habit, extending over a period of years, that gives permanency to associations. There is nothing in the oft-quoted ideomotor actions to warrant the assumption that every incoming impression goes at once into some form of movement, and that this is based upon the structure of the nervous system.

In speech-motor reactions it is probable that the same habit principle obtains. Curtis[2] fastened a tambour to the larynx in such a way as to record slight movements there. He could obtain records when his subjects read silently or recited mentally. Nevertheless, twenty per cent. of his subjects gave no records of any movements.

The motorist is not on safe ground when he retreats to the position that impressions *eventually* issue into action. This assumes that an impression may persist for a long time and then assert its motor rights. Some impressions may do that. Many of them fade out, merge into a type or into some other impression. We can no longer hold the fantastic belief that every impression leaves an indelible mark on the soul. The few striking instances of memories evoked in delirium, or hypnosis, which bring forth events forgotten for years, do not prove a miraculous retention on the part of the cortex. If every impression goes forth into expression, then each tree, twig, stone, pebble, sparrow, and cloud that flits past the car window from Boston to San Francisco will leave a mark on the obser-

[1] M. A. Tucker, 'Comparative Observations on the Involuntary Movements of Adults and Children,' *Amer. J. of Psychol.*, 1896–97, 8, 394–404.

[2] *Amer. J. of Psychol.*, 1900, 11, 237–239.

ver's nervous system which will later issue in some form of behavior! This *must* happen despite the fact that the observer forgets these trifles as quickly as he sees them!

A less fantastic and more important feature of the motorist's program seeks to maintain the doctrine that conscious states are conditioned, or determined, by motor activities. This does not mean that the kinesthetic sensations of movements accompanying perceptions blend with these perceptions and give a certain character to them. That is an old, an obvious fact. The conception is derived from the belief that sensory, central, and motor processes are one process. All three function together. It is an extreme statement of the unity of the nervous system and of conscious activities. Its over-emphasis distorts a valuable truth and a serviceable conception.

The strongest arguments for the contention are to be found in the relations between the thought and speech processes. So intimately is speech bound up with ideation and, indeed, perception that it would seem impossible to separate them. If the motor discharge conditions the conscious content in any activity it must do it here. If the motor areas are indispensable to the neural basis of any conscious state, they are indispensable here. This is so apparent that Watson pins his faith to the integrity of the process. A man lacking speech-motor action is a 'crucial case' for behaviorism. Watson believes that such cases are 'extremely unlikely' to occur; they are rare, but they do occur. Pure motor aphasia calls for a disorder in such a minute and restricted area that few cases are recorded. Usually the disturbance involves other parts and a number of sensory and motor defects accompany the aphasia. Rare though such cases are, they are sufficiently well known and adequately described for the purpose in hand. Dejerine[1] states that "the sole phenomenon consists in the impossibility of the articulation of sounds in all their modes, but all other qualities of language are intact and the interior language functionates as in the normal individual." Moutier says the condition is one in which the patient finds it impossible to communicate with his entourage, the tongue does not obey

[1] Quoted by Dercum, *J. of Nerv. and Ment. Disease*, 1914, 41, 137–141.

the will. The thoughts in his mind are very clear and his words precise, but he cannot exteriorize them at all. "There is present but one symptom and that is the suppression of articulate language." Dercum describes such a case. His patient showed no sensory losses at all. "He had apparently complete motor aphasia. He was unable to make the slightest articulate sound. . . . At the same time the patient's understanding of spoken speech and comprehension of what was said to him was perfect. . . . Speech comprehension was completely preserved. Further, the patient could read written or printed matter and carry out instructions perfectly." There was no agraphia, he could write spontaneously and from dictation. Dr. Dana[1] reports three cases on his lists. Of one he says: "The patient cannot say a word, or repeat, or ejaculate, or count. But he writes, reads, understands, and has good general intelligence." In short, here we have several cases of the loss of the speech mechanisms and no 'serious limitation' in the patient's 'thought processes.' The elimina, tion of the motor factor has not disturbed the central and sensory processes. The perception of words has not been impaired, much less inhibited, by the inability of the incoming impulse to discharge into the speech motor area.

If the motor discharge into the larger musculature of the trunk and limbs were necessary to normal conscious life we should expect to find curious distortions in the thought and feelings of anyone suffering from a motor paralysis. Indeed-from the certainty which runs through the motor theories, we should be able to map out definite fields of consciousness which would be impaired as a paralysis spreads over legs, trunk and arms. As a matter of fact nothing of the kind occurs. Cases may be found where only the spinal motor tracts are involved. In these there is no obliteration of large parts of the thought and emotional life of the patients—certainly no impairment of perception.

It seems probable that the line of attack will be shifted by the more ardent of the motorists from overt movements of the musculature to less accessible regions, where observation

[1] *N. Y. Med. Journal*, Aug. 10, 1907.

and experiment cannot follow. The glands offer a tempting opportunity. If we *must* have every incoming impulse and every central nervous process issuing in a motor discharge, why not assign the splendid duty to that famous and historic organ, the pineal gland?

DISCUSSION

FURTHER LOGICAL ASPECTS OF THE BINET SCALE

BY TRUMAN L. KELLEY

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Due to what the writer considers is the substantial progress made by Otis[1] toward the development of a theoretically sound procedure in standardizing and using tests of the Binet type, as opposed to the altogether empirical, not to say haphazard, methods characteristic of past work along this line, he feels that a criticism and supplementing of Otis's conclusions may be helpful.

Otis concludes with reference to purpose 1 that a test is standard for the age at which 50 per cent. pass the test. His summary, paragraph 2, is a logical sequence to this definition of "standard."

"If tests are grouped together in age groups such, for example, that the tests in age group X. are standard for ages ranging from $9\frac{1}{2}$ to $10\frac{1}{2}$, or such that the number of tests standard for ages above ten is probably equal to the number standard for ages below ten (as would be the case by chance if the tests were placed roughly as near ten as possible), or if the tests were all standard for exactly the age of ten, then the proper way to score them would be to give the child tested a score of ten years in mental age if the number of tests passed equalled the number to and including one half of the ten-year tests, and the same for the other years. If, however, the tests are to be scored as at present, giving a score of ten years of mental age only when all the tests to and including those in group X., or an equivalent number, are passed, then group X. must be composed of tests standard for ages ranging from nine to ten, or all standard for nine and a half."

This entirely sound procedure is quite at variance with the practice of the devisers of tests of the Binet type who have used 75 per cent. correct of the responses of a given age as a criterion for considering the test as standardized for that age. Otis explains the fact that their results have been at times approximately correct

[1] Arthur S. Otis, 'Some Logical Aspects of the Binet Scale,' PSYCHOL. REV., 1916, 23, Nos. 2 and 3.

as due to the fact that these workers had inadvertently lost track of a half year in making their calculations; in brief, that they had assigned tests passed by 75 per cent. of children of average age 10.5 to a group the passing of which gave the child credit for mentality 10.0. So obvious an error is hardly to be attributed to an oversight on the part of the experimenters concerned. If they did resort to the procedure charged we may well believe it was because the empirical results obtained were more in harmony with the known mentality of the children tested than would have been the case by adding .5 of a year.

It is not argued that their procedure was logically sound, that would be maintaining more than in all probability even they would claim, but it is proposed to show that to secure a group of five tests the passing of all of which is to be expected from 50 per cent. of children of age 10.0, does require that each of the tests separately be passed by some per cent. greater than 50.

If there are five tests *a*, *b*, *c*, *d*, *e* for age 10.0 and if each of the tests separately is passed by 50 per cent. of the children of this age, then only in case every child who passes test *a* also passes each of the others and vice versa does it result that 50 per cent. of the 10.0-year-old children pass the entire group of five tests. Only in case the correlation between each of the tests in the group and all the others is perfect, or 1.00, does 50 per cent. passing for each test singly result in 50 per cent. passing the entire group. A group of tests perfectly correlated would have no significance over and above a single one of the group, for the one test alone would divide the individuals into exactly the same divisions as would the entire group.

If the five tests are each passed by 50 per cent. of a given age and if each test is uncorrelated—shows .00 correlation—with each of the other tests then out of a number taking test *a* 50 per cent. will pass; out of the 50 per cent. that pass one half, or 25 per cent. of the total will pass test *b*; etc., until we have $3\frac{1}{8}$ per cent. only passing all five tests. This result may be obtained more readily by simply evaluating $(\frac{1}{2})^5$.

If we have five uncorrelated tests and wish to find the per cent. that must pass each separately if 50 per cent. are to pass the group entire, we may do so by means of the equation, $x^5 = .50$. Solving $x = .8705$. Omitting consideration of the relationships that would maintain if some of the correlations between the five tests were negative, for it is hardly likely that such would ever be the case,

it is apparent that for five tests as a group to be passed by 50 per cent. of the children of a given age, some per cent. greater than 50 and less than 87 must pass each of the tests separately, the exact per cent. needed being dependent upon the intercorrelations between the five tests—the greater these correlations are the lower the requisite per cent. and vice versa.

These considerations show the futility of attempting to establish a single per cent. which can be reliably accepted as standardizing a test if that test is to be grouped with others to yield 50 per cent. of passes for the entire group. Any per cent. established would be true in case certain intercorrelations held, and as these intercorrelations are by no means constant a single per cent. cannot be established as the standard. Suppose that it had been determined, as it might be by means of very laborious calculations, that five tests, each showing the same definite correlation *k* with each of the others, must separately be passed by 75 per cent. of children of age ten in order that 50 per cent. of such children pass the entire group. Suppose further that this 75 per cent. criterion is used to put five tests showing mutual intercorrelations of less magnitude than *k* into a single age group. The result will be that less than 50 per cent. of the children of the age considered will pass the group entire and accordingly the group is too hard. This is probably the situation that actually maintains in the 11 and 12 year Binet groups and in part accounts for the fact that exerimentally they have been found to be too difficult, though from the standpoint of percentage of children passing each test they are standardized upon the same basis as the tests in other age groups.

A very simple way to circumvent the difficulties mentioned is to standardize tests upon the basis of 50 per cent. correct responses by children of the age for which standardized and then give a child credit for this age if he passes 50 per cent. of the tests in the group. (Otis reaches this same conclusion by a somewhat different approach.) With such an arrangement no systematic tendency to pass more or less than 50 per cent. of normal individuals of the age for which the tests were standardized would result as a consequence of high or low intercorrelations between the tests in the group. Though no such constant error would be introduced by having a group of very highly intercorrelated tests, such tests would result in mental age determinations having a large probable error. For these reasons and others[1] adopting 50 per cent. right answers

[1] I have shown in "A Simplified Method of Using Scaled Material," *School and*

by children of a certain age as the criterion for acceptance of the test for that age is to be recommended. The facts just presented lead to a second criticism of Otis's recommendations.

If there are five tests, standardized as recommended, for the age 10.0 group and individuals who pass 0, 1, 2, 3, 4, 5 of them respectively are considered to be successively of higher mentality, a distribution like that shown in Fig. 1 would represent the situa-

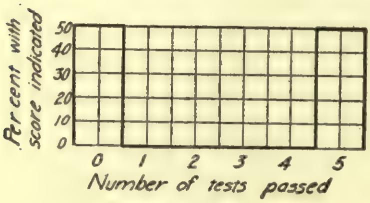

FIG. 1.

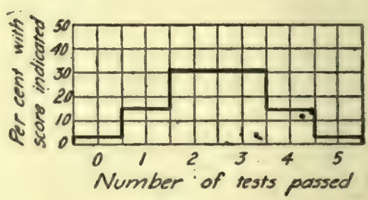

FIG. 2.

tion in case the five tests are perfectly correlated and are given to a number of children of mentality 10.0. There would be 50 per cent. of complete failures and 50 per cent. of total successes, giving an average success of $2\frac{1}{2}$ tests, or 50 per cent. right, and the standard deviation of this distribution would equal the distance covered by $2\frac{1}{2}$ tests which would be .5 of a year if the age range to which the tests were applicable was one year. On the other hand, given a zero correlation the distribution would be as shown in Fig. 2. The average accomplishment is the same as before, but the standard deviation of the distribution is only 1.118, or .2236 of a year, showing much greater reliability in this case than in the former. It may be stated[1] that if the five tests show some positive intercorrelation greater than 0 and less than 1, the standard deviation of the corresponding distribution would lie between .5 and .2236, the smaller the intercorrelations the closer to .2236. These considerations aid in establishing the criteria as to what constitutes a good group of five tests.

Otis proposed and answered the question as to what constitutes a good test, but in doing this he assumed that he had at the

Society, 1916, **4**; 79–80, that the determination of the difficulty of questions such that a child can pass 50 per cent. of them is more reliable than the determination of a difficulty corresponding to some other per cent.

[1] The rigorous proof of this involves the calculation of the standard deviations of correlation solids of five independent variables. The writer has not attempted this very laborious task but he has established the principle for the case of two independent variables.

same time laid down the criteria for determining the best tests when used as a group. He is in error in this and for fundamentally the same reason that he was in error in his understanding of why tests standardized upon the 75 per cent. basis do give, when used in a group as in the Binet test, approximately 50 per cent. of passes for the group of five. The problem at hand is one which is, in the statistical sense, best solved by partial correlation and not by total correlation.[1]

Otis recognizes that the determination of the merit of a group of tests must rest upon their relationship with an outside standard. Having then established by as reliable means as possible, other than by means of the tests themselves, the degrees of intelligence possessed by a number of ten-year-old individuals, a large number of tests may be given to these subjects for purposes of standardization. The scores made by each of the ten-year-olds, for any five of these tests may be correlated with the established measures of intelligence for these same individuals and that group of five which yields the highest correlation will be the best group. This procedure is fundamentally different from that of determining the merit of a group by means of the individual correlations of its parts with established measures of intelligence. Unless the intercorrelations between tests are taken into consideration each test of the five will likely measure the same, or nearly the same, trait—some trait which correlates highly with the established measure of intelligence—and the group of five will fail to measure the all-around mental make-up of the individual. The procedure proposed has been with reference to securing the best group of five tests for a given age. In actual practice it would be better to extend the group to include all tests likely to be given to a single age.

In giving the requirements of a test the present writer would supplement, by the addition of the words in italics, the statement made by Otis on p. 166:

"Thus, if 50 per cent. of ten-year-olds pass a test, the degree of merit of the test as a test of intelligence, *when used in conjunction with other tests*, is measured by the degree in which those who pass it are identical with those whose intelligence is known to be above ten-year intelligence *and also by the smallness of its correlations with the other tests with which it is to be used.*"

[1] The value of this method has been more fully exemplified in Truman L. Kelley, 'Educational Guidance,' 1914, where the grades of several tests have been combined into a single grade which resulted in the highest possible correlation with the trait dealt with.

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The Psychological Review

A MEASUREMENT OF THE LANGUAGE ABILITY OF DEAF CHILDREN

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Language Ability.—In the education of the deaf great emphasis is naturally laid upon the acquisition of language. The deaf child is almost entirely dependent upon formal education in acquiring language. Unlike hearing children he does not live in a 'language environment,' he does not acquire unconsciously a great number of words and phrases, and he is therefore dependent upon the instruction of the class-room for the development of language. The literature dealing with the education of the deaf abounds in references to the importance of the acquisition of language, and it may truly be said that of all subjects of instruction in our schools for the deaf, this subject is the one to which most attention is paid and to which most time is devoted. We quote from a course of study prepared by Robert Patterson, principal of the Ohio Institution for the Deaf, published in 1891, a book to which we shall refer later in discussing the Trabue Language Scale. The first words of the preface, written by James W. Knott, superintendent of the Institution, are: "In this school, as in all deaf-mute schools in America, the prime object to be held in view by every teacher, at all times, is to teach the pupils a correct and easy use of written English language. In the world at large, the deaf-mute must depend almost wholly upon his knowledge of the written language

for his means of communication with speaking-hearing people. Without a good knowledge of written English, he is helpless. With such knowledge he can transact the ordinary business of life, and can enjoy that greatest of all pleasures of solitude—reading." The oralist of to-day might substitute or add spoken language, but in either case the emphasis is thrown upon language work. We have not quoted the above as coming from any great authority in the education of the deaf, but merely owing to the fact that we shall refer shortly to this same manual in regard to one method of teaching language which has been so greatly emphasized in the preface of the book. This attitude towards language work has largely dominated the education of the deaf and is a common one at the present time. Johnson has recently emphasized the importance of language in the following words: "But what are these fundamentals? One and one only. Language and then language—spoken, spelled or written—and the power to read, and the power to understand what is read."[1]

Since language work is so important, it was thought that a measurement of the language ability of the deaf might throw some light upon the factors involved in the development of language ability among the deaf. The measurement of language ability might be roughly divided into the measurement of ability to comprehend language on the one hand, and on the other the ability to compose or construct language. The former ability might best be measured by such tests as Thorndike's[2] 'Reading Scales,' or Kelley's[3] 'Silent Reading Tests,' or Woodworth and Wells[4] 'Directions Tests.' This last test was used by Pintner and Paterson[5] to measure the deaf child's ability to comprehend printed

[1] Johnson, R. O., 'What are the Fundamentals?' *American Annals of the Deaf*, 1916, 61, 92–95.

[2] Thorndike, E. L., 'The Measurement of Ability in Reading, Preliminary Scales and Tests,' *Teachers College Record*, 1914, 15, No. 4.

[3] Kelley, F. J., 'The Kansas Silent Reading Tests,' *J. of Ed. Psych.*, 1916, 7, 63–80.

[4] Woodworth, R. S. and Wells, F. L., 'Association Tests,' Psychol. Monog., 1911, 13, No. 57.

[5] Pintner, R. and Paterson, D. G., 'The Ability of Deaf and Hearing Children to Follow Printed Directions.' To be published shortly in the *Pedagogical Seminary*.

language. Composition ability might best be graded by the Hillegas,[1] or by the Harvard-Newton[2] Scale. A combination of the two factors of comprehension and composition is supplied by the Trabue Language Scale,[3] and it was this test that was chosen in the present work to measure the language ability of deaf children. It was thought that, since only one single test could be employed, the Trabue Language Scale would give the best all-round measurement of language ability.

The Trabue Language Scale is based upon what is generally known in psychology as the Ebbinghaus Completion Method, that is, the filling in of the correct word in a blank space in a sentence. Ebbinghaus[4] seems to have been the first to use this method as a psychological test. It is interesting to note, however, that this device of filling in blanks is a common one among teachers of the deaf as an exercise in language work and one that has been in use at least since the year 1891. In the Course of Study of the Ohio Institution for the Deaf (1891), referred to above, we find this as one of the exercises recommended for first-year work. To quote: "As a means of getting the children into the habit of thinking for themselves, which will ingrain the principles of language in their mental fiber, give them abundant practice in finding appropriate words to fill the blanks; as,

....................sits.
A boy ....
A cat sees a ....
Mary .... Edith."

In Second, Third and Fourth Grade work "filling blanks," as this method is called, is recommended as an appropriate exercise in language work.

It will be obvious from this that the Trabue Language

[1] Hillegas, M. B., 'A Scale for the Measurement of Quality in English Composition by Young People,' *Teachers College Record*, 1912, 13, No. 4.

[2] Ballou, F. W., 'Scales for the Measurement of English Compositions,' *Harvard-Newton Bulletins*, Harvard University, 1914, No. 11, p. 93.

[3] Trabue, M. R., 'Completion-Test Language Scales,' *Teachers College Contributions to Education*, 1916, No. 77.

[4] Ebbinghaus, H., 'Ueber eine neue Methode zur Prüfung geistiger Fähigkeiten und ihre Anwendung bei Schulkindern,' *Zsch. f. Psychol.*, 1897, 13, 401–459.

Scale is eminently fitted for testing the language ability of deaf children. The form of the test is not new to them and therefore any initial strangeness due to the peculiar form of a psychological test will not enter into this test. Indeed, in this respect they will have an advantage over the hearing children, for we do not believe that this method is commonly in use in language work in hearing schools.

The Method of Procedure.—Scale *A* of the Trabue Language Scale was used in this study. It consists of twenty-four sentences grouped in pairs of comparatively the same difficulty. The pairs become progressively difficult as we proceed from the first to the last pair.

The subjects tested were 570 deaf pupils, 242 in the Kentucky State School for the Deaf, and 328 in the Ohio State School for the Deaf.[1] The method of procedure in giving the test was to give each pupil in the class the preliminary practice sheet, as recommended by Trabue for work with small children. This practice sheet contains four simple sentences, in each of which there is a blank space which requires an appropriate word. The experimenter wrote the first sentence on the board and asked the class what they would write in to complete the sentence. In most cases the correct answer was forthcoming, but if not, the experimenter wrote in the correct word himself and then instructed the children to do this on their sheets. It was emphasized again and again that only one word was to be written in each blank space. The same procedure was followed with the other three sentences. The practice sheets were then collected, and the test sheets proper were distributed. The first page of the sheets of Scale *A* contains three practice sentences and the children were told to fill in appropriate words. The name, grade, and age of the child is also asked for on this first page and in the same method of filling in blanks. The pupils were allowed as much time as they required to fill in this first page, and while they were at work

[1] The writers take great pleasure in acknowledging here the kindness and courtesy shown to them by Mr. Jones, superintendent of the Ohio State School for the Deaf, and Mr. Augustus Rogers, superintendent of the Kentucky State School for the Deaf.

the examiner went around the class helping those who encountered any difficulty. After this was completed, a signal was given and the children turned over to page two of the test sheet and began the test proper. At the end of 15 minutes they were all required to stop. This time limit of 15 minutes is in most cases too long for deaf children. Nearly all of them had finished long before the 15 minutes had elapsed, owing to the fact that most of them were unable to accomplish very much. With those who possess a good mastery of language, this period of time is adequate for them to complete the whole test.

The method of scoring adopted was that used by Trabue,[1] *i. e.*, a score of two is allowed for a good sentence, a score of one for a fair completion, and no score for an inadequate completion. The examples given by Trabue were referred to constantly in order to determine the score for each sentence. The sum of the scores for all the sentences attempted represents the final score for the child.

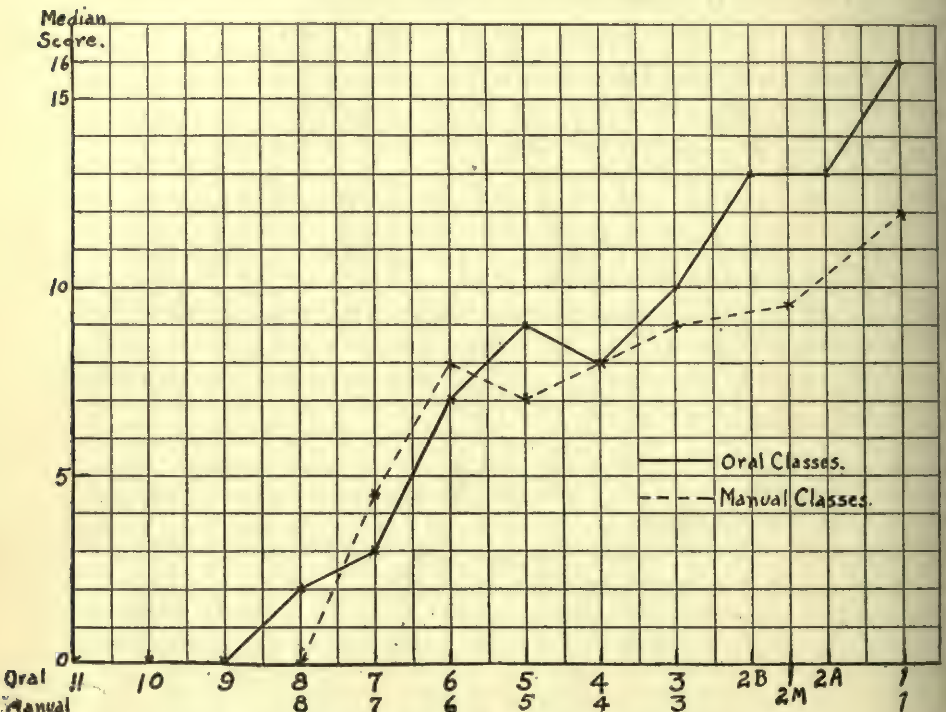
The Results

Comparison by Classes.—The first tabulation of the results was made according to classes. The two schools in question are combined schools. Two methods of instruction are employed, namely, the oral method which teaches by means of speech and lip-reading, and the manual method which makes use of signs and the manual alphabet. The results of these two groups of classes have been kept separately and will be referred to as oral and manual classes respectively.

Graphs I. and II. show the class medians for the two schools for each class and for each of the two methods of instruction. In Graph I. (the Kentucky School) it will be noted that with two exceptions the manual classes are somewhat below the oral classes. Only in the two upper classes is the difference in language ability very marked. The curve for the oral classes shows one drop at the fourth class, but it must be borne in mind that the fourth and fifth classes are

[1] Trabue, *op. cit.*, 79–115.

parallel classes doing the same year's work. The medians of these two classes would suggest that there is one relatively slow and one relatively advanced class doing the same year of work. The curve for the manual classes shows a drop at class five. This class is doing a higher year of work than

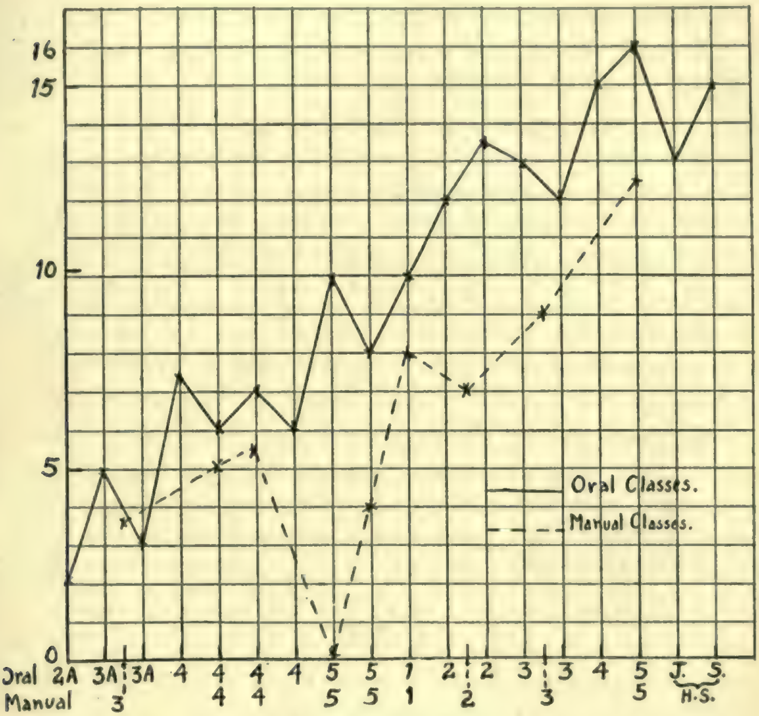


Graph I. Showing Medians of the Classes in the Kentucky School

class six. It would seem from the median of class six that this class is good in language ability, since it is better than class five and equal to class four, and both of the latter classes are doing more advanced work than class six.

In Graph II. (the Ohio School) the numbering of the classes is different from that in Kentucky. In this school all classes doing the same year of work are given the same numbers. Both oral and manual curves show decided fluctuations from class to class. Following the oral curve we note that there are two third primary classes of distinctly

different language ability, in other words a poor and a good class. This division into a poor and a good group in the third primary would indicate a good classification of the children. The medians of the four fourth primary classes vary slightly (*i. e.*, the medians are 7.5, 6, 7, 6), but are all close enough together to suggest that, for language work at least, a much better division might be possible whereby the best pupils might be in one section, the poorest in another



GRAPH II. Showing Medians of the Classes in the Ohio School

and the average ones in the two remaining sections. There are 41 pupils in these four classes and if a division as suggested above were to be made, we would have, first, a poor section of 10 with scores from 1 to 4 with a median about 2.5; second, a medium section of 10 with scores from 4 to 6 with a median of 5; third a medium section of 11 with scores from 6 to 7 with a median of 6.5; and lastly a bright section of 10 with

scores from 8 to 12 with a median of 10. The two sections of the fifth primary show a good and a weak section, as is also the case with the two sections of the second intermediate. The two third intermediate sections are both decidedly poor and fall below one of the sections of the second intermediate. There is a peculiar drop in language ability in the two high school classes. It has been suggested that this is due to the entrance into these classes of pupils from the manual classes.

The curve for the manual classes is uniformly below that of the oral. From the results of other tests previously reported,[1] we believe that in this school the difference can be accounted for by the fact that the brighter pupils tend to be chosen for oral work. Following the manual curve we note that the third manual does as well as one of the sections of the third oral, but falls decidedly below the other. The two sections of the fourth manual if divided according to language ability would give a slow section of 12 pupils with scores ranging from 0 to 4 with a median of 2, and a better section of 11 pupils with scores from 5 to 9 with a median of 6. It appears to the writers that division of classes into sections according to ability as measured by tests might decidedly improve the classification and be a decided aid to efficient teaching. The two sections of the fifth manual are evidently well classified into a poorer and a better group, although as far as language is concerned, it seems strange that the one section should be ranked as a fifth manual in as much as its median language ability is zero, *i. e.*, the lowest of any manual class. The second intermediate manual drops below the first intermediate in language ability.

Comparison by Years of Work.—The course of study in the two schools extends over thirteen years of work. The next comparison, therefore, that was made, was a comparison of

[1] Pintner, R. and Paterson, D. G., 'Learning Tests with Deaf Children,' Psychol. Monog., 1916, **20**, No. 88.

Pintner, R. and Paterson, D. G., 'A Class Test with Deaf Children,' *J. of Educ. Psychol.*, 1915, **6**, No. 10.

Pintner, R. and Paterson, D. G., 'A Comparison of Deaf and Hearing Children in Visual Memory for Digits,' *J. of Exp. Psychol.* To appear shortly.

the language ability of the pupils grouped according to the year of work. These results are given in Tables I., II., and III.

Table I. gives the data for the oral pupils for each school. The vertical columns show the year of instruction, from the

TABLE I

ORAL PUPILS: BY YEARS OF WORK

		Years of Work											
		13	12	11	10	9	8	7	6	5	4	3	2
Number Tested..	Ohio.	—	—	9	15	22	25	12	28	41	22	13	—
	Ky. .	—	9	—	—	—	23	12	22	11	21	26	13
Median .	Ohio.	—	—	16	15	12.5	13	10	8.5	6	5	2	—
	Ky. .	—	16	—	—	—	13	10	9	9	2	0	0
25 percentile	Ohio.	—	—	13.25	14.0	10	9.25	6	7	4	1	0.25	—
	Ky. .	—	12	—	—	—	11	10	7.15	8	2	0	0
75 percentile	Ohio.	—	—	20.75	19.75	16	16.5	13	10	7.75	7.5	4	—
	Ky. .	—	24	—	—	—	14	11.5	11	10.25	4	0.5	4.5
Quartile.	Ohio.	—	—	3.8	2.8	3.0	3.6	3.5	1.5	1.9	3.3	1.9	—
	Ky. .	—	6	—	—	—	1.5	1.2	1.7	1.2	1	0.25	2.2
Grade Ability..	Ohio.	—	—	4.5	4.25	3.7	3.8	3.3	3.1	2.5	2.25	−2	—
	Ky. .	—	4.3	—	—	—	3.8	3.4	3.2	3.2	−2	−2	−2

thirteenth down to the second. It was found impracticable to test any of the first-year pupils. The first two horizontal columns give the number of pupils in each year of instruction. The second two give the medians. Here it will be noted that there is a much more uniform increase from year to year than was observed in the graphs showing the medians of the various classes. On the whole the medians show little difference in language ability in the two schools. The greatest difference occurs in the fourth year, where the median for Ohio is 5 and for Kentucky only 2. The next two horizontal columns show the 25 and 75 percentiles. The 25 percentile is the highest score made by the lowest 25 per cent. of the group while the 75 percentile represents the highest score attained by 75 per cent. of the group. The 25 percentile seems on the whole to be somewhat lower for Ohio than for Kentucky; and the 75 percentile seems on the whole somewhat higher for Ohio than for Kentucky. This indicates a wider range in the Ohio groups and this is borne out by the quartiles in the next horizontal columns. With one minor

exception these are all higher for Ohio than for Kentucky. The last two columns compare the median for each year of work with the median grade ability of hearing classes as tested by Trabue. In these columns the grade ability of hearing children which corresponds to the medians for the deaf is given. Deaf children in the tenth, eleventh, and twelfth years of work exceed slightly the median language ability of fourth-grade hearing children. A grade ability of − 2 indicates that the median for the deaf has not reached the median for second-grade hearing children. This is the lowest grade in hearing schools reported by Trabue. This comparison with hearing children brings into sharp relief the difficulty that deaf children experience in the acquisition of language and will be further commented upon below.

Table II. gives similar data for the manual pupils arranged according to years of work. The medians for these pupils

Table II

Manual Pupils: By Years of Work

		Years of Work											
		13	12	11	10	9	8	7	6	5	4	3	2
Number Tested..	Ohio.........	—	—	12	—	13	12	13	26	23	16	—	—
	Ky...........	—	7	—	8	14	16	15	18	—	14	—	13
Median.	Ohio.........	—	—	12.5	—	9	7	8	3	5	3.5	—	—
	Ky...........	—	12	—	9.5	9	8	7	8	—	4.5	—	0
25 percentile.	Ohio.........	—	—	6	—	7	6	6	0	2	1	—	—
	Ky...........	—	10.5	—	8	8	6.5	5.5	6	—	2	—	0
75 percentile.	Ohio.........	—	—	14	—	10	8	9.75	5	6.25	5	—	—
	Ky...........	—	14	—	11	12	9.5	8	10	—	6	—	0
Quartile.	Ohio.........	—	—	4.0	—	1.5	1.0	1.9	2.5	1.6	2	—	—
	Ky...........	—	2.2	—	1.5	2	1.5	1.2	2	—	2	—	—
Grade Ability..	Ohio.........	—	—	3.77	—	3.17	2.75	3	−2	2.25	−2	—	—
	Ky...........	—	3.6	—	3.2	3.2	3	2.75	3	—	2	—	−2

do not show quite such a steady increase from year to year as is the case with the oral pupils. The quartiles for Ohio are not so large as for the oral pupils, indicating that the range of ability with the manual pupils at each year of work is not so great. The comparison with the grade ability of hearing children shows that no group of manual pupils reaches a fourth-grade ability in language. The somewhat inferior performance of the manual pupils as noted on Graphs I.

and II. is brought out again in the comparison shown in this table.

Table III. shows the combined results of both schools divided as to oral and manual pupils. Though the number

TABLE III

OHIO AND KENTUCKY COMBINED

Oral Pupils: By Years of Work

	Years of Work											
	13	12	11	10	9	8	7	6	5	4	3	2
Number..........	—	9	9	15	22	48	24	50	52	43	39	13
Median..........	—	16	16	15	12.5	13	10	9	7	3	0	0
25 percentile.....	—	12	13.25	14.0	10	11	6	7	5	1	0	0
75 percentile.....	—	24	20.75	19.75	16	15	13	10.75	8	6	2	4.5
Quartile..........	—	6	3.8	2.8	3.0	2.0	3.5	1.9	1.5	2.5	1.5	2.2
Grade ability....	—	4.5	4.5	4.25	3.7	3.8	3.3	3.2	2.75	−2	−2	−2

Manual Pupils: By Years of Work

Number..........	—	7	12	8	27	28	28	44	23	30	0	13
Median..........	—	12	12.5	9.5	9	8	7	5	5	4	—	0
25 percentile.....	—	10.5	6	8	7	6	6	1	2	2	—	0
75 percentile.....	—	14	14	11	10	9	9	8	6.25	5.5	—	0
Quartile..........	—	2.2	4.0	1.5	1.5	1.5	1.5	3.5	1.6	1.8	—	0
Grade ability....	—	3.6	3.8	3.2	3.2	3	2.75	2.25	2.25	2	—	−2

under each year of work is not great, the medians may serve as tentative norms of language ability for oral and manual children. In a combined school an oral pupil in the ninth year of work might be expected to make a score of 12.5 and a manual pupil in the same year a score of 9, and so on. It will be noted that with both groups of pupils the medians show an almost steady increase from year to year. Looking at the last horizontal column giving the comparison with hearing schools, we might say that the oral pupil advances from second-grade ability in his fourth or fifth year of schooling to somewhat above fourth-grade ability in his twelfth year of instruction. A manual pupil's progress seems to be slower, advancing only to somewhat above third-grade ability in his twelfth year of instruction.

Table IV. shows the combined results for all the 570 children tested, and we may regard these as tentative norms or all deaf, regardless of method of instruction. The in-

TABLE IV

ALL DEAF: BY YEARS OF WORK

	Years of Work												All Deaf
	13	12	11	10	9	8	7	6	5	4	3	2	
Number	7	35	21	23	49	76	52	94	75	73	39	26	570
Median	15	13	14	14	10	11	8	7.5	6	3	0	0	7
25 percentile	13.5	11	11.25	9.75	8	8	6	4.5	4	2	0	0	4
75 percentile	26.0	15.75	17.5	18.5	13	14	10	10	8	6	2	2	11
Quartile	6.7	2.4	3.1	4.4	2.5	3	2	2.8	2	2	1	1	3.5
Grade ability	4.25	3.8	4.0	4.0	3.3	3.5	3	2.8	2.5	−2	−2	−2	2.75

crease in the median from year to year is not quite uniform and this would lead us to suspect a lack of definite standards of work required at the different institutions and in the different divisions (oral and manual) in the same institution. In language ability at least no very definite standard seems to be set up to which the pupil must approximate before he is allowed to go on with the next year's work. This fact is borne out by the large quartiles, and they are particularly large in the upper years. We would raise the question as to the desirability of setting up more definite standards of attainment for each year of work in the interest of more efficient instruction in any one school, and of a more definite understanding between schools as to what a year of work in any subject means, with a view of ultimately bringing about a better standardization of schools. Comparisons between schools would in this way be better and more justly made, and the transfer of a pupil from one school to another would be effected with less waste of time in determining his proper grade.

The comparison of the deaf child with the hearing child, as shown on the last line, brings out strikingly the poverty of language attainment among the deaf. After thirteen years of instruction the language ability of the average deaf child does not reach that of a fifth-grade pupil in our hearing schools. There are, as we shall see later, quite a number of cases that go beyond this ability, but, nevertheless, the average remains at this low score. Up to the fourth year of instruction the average ability does not rise to that of a second-grade hearing child. From this point on, it creeps

slowly up to third- and fourth-grade ability. This comparison shows better than any we have seen the great difficulty the deaf must experience in the acquisition of the language of the hearing. In spite of the great emphasis placed upon language in the teaching of the deaf, the progress of the pupil is incredibly slow. We must remember, of course, that the acquisition of language by a deaf child is somewhat analogous to the acquisition of a foreign language by a hearing child. If the data were at hand, it would be interesting to compare the rate of acquisition of language by the deaf with the rate of acquisition of a foreign language by a hearing child not living in the country in which the foreign language is spoken.

Comparison of Adventitious and Congenital Deaf.—The comparison between the adventitious and congenital is made for the two schools combined, since there is nothing to be gained from this comparison for each of the two schools taken separately. Table V. gives the distribution for these

TABLE V

ALL DEAF: COMPARISON OF CONGENITAL AND ADVENTITIOUS CASES

Congenital Cases: By Years of Work

	Years of Work												All Cong.
	13	12	11	10	9	8	7	6	5	4	3	2	
Number...	1	16	6	7	16	31	22	42	33	37	22	17	250
Median....	14	13	12	11	9	11	8	7.5	6	3	0	0	7
25 percentile...	—	11	—	—	7	8	7	5	4	1.25	2.5	0	3
75 percentile...	—	14	—	—	11	13	11	9.5	8	5.75	0	0	10
Quartile ...	—	1.5	—	—	2	2.5	2	2.3	2	2.3	1.3	0	3.5
Grade ability...	4.0	3.85	3.7	3.5	3.2	3.5	3	2.8	2.5	−2	−2	−2	2.75

Adventitious Cases: By Years of Work

	Years of Work												All Ad.
	13	12	11	10	9	8	7	6	5	4	3	2	
Number...	6	19	15	12	30	45	29	50	38	34	14	9	301
Median....	15	13	14	16	11	11	8	8	6.5	4	0	0	8
25 percentile...	—	10.5	11	10	9	7.5	6	4	4	1.5	0	0	4
75 percentile...	—	18.25	20	22	13.5	14.5	10	10	8	6	2	2	12
Quartile ...	—	3.8	4.5	6	2.3	3.5	2	3	2	2.3	1	1.0	4
Grade ability...	4.25	3.8	4.0	4.5	3.5	3.5	3	3	2.6	2	−2	−2	3

TABLE VI

ALL DEAF, ARRANGED ACCORDING TO TIME WHEN DEAFNESS OCCURRED

	Cong.	Unknown	Ac.	Before 1	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Number	250	19	20	45	85	65	32	19	11	10	1	0	2	3	2	2	2	1	1
Median	7	6	8.5	8	7	6	9	10	14	12	10	—	20	17	20	15.5	35	26	23
25 percentile	3	1.5	6	0.5	4	4	4	7.5	6	8.5	—	—	—	—	—	—	—	—	—
75 percentile	10	13.5	11	10	11	11	13	17	15.75	26.5	—	—	—	—	—	—	—	—	—
Quartile	3.5	6.0	2.5	4.8	3.5	3.5	4.5	5.2	4.8	9.0	—	—	—	—	—	—	—	—	—
Grade ability	2.75	2.5	3.1	3	2.75	2.5	3.2	3.3	4	3.7	3.3	—	5.7	4.75	5.7	4.3	11.0	7.25	6.5

two groups of deaf children arranged as before according to years of instruction. An inspection of the medians for the two groups shows that at every year of work the adventitious group is either better or at least equal to the congenital group. In the twelfth, eighth, seventh, third, and second year of work the medians for the two groups are the same; in all the other years the median of the adventitious is higher than that of the congenital group. This somewhat better showing of the adventitious is, of course, reflected in a somewhat higher grade ability in comparison with the hearing child. The median score of all the congenital cases taken together is 7, whereas the median for the adventitious is 8.

Time of Occurrence of Deafness.—The slightly better showing of the adventitious group leads us to raise the further question as to whether the time at which deafness occurred has any relation to the language ability of the child. It is generally held by teachers of the deaf that a few years of hearing or even a few months has a decided influence for the better in the acquisition of language by the deaf child. Table VI. gives the results of our test from this point of view. The first vertical column gives the results for all the congenital cases; the second column, headed 'unknown,' for those cases where the cause of deafness, whether congenital or adventitious, was not known; the third column for the acquired cases

where the year at which deafness occurred was doubtful or unknown. The remaining columns show a distribution according to the year at which deafness occurred. 'Before I.' means all cases of deafness occurring before the first birthday. 'One' means cases occurring between the first and second birthday and so on. In the upper groups, showing the occurrence at age seven or later, there are relatively few cases in any one group. An inspection of the medians shows little difference between the congenital group and the adventitious groups up to age four. In these first eight groups on the table, the median ranges from 6 to 10. Ages three and four show a very slight increase above the score of the congenital group, an increase which may be due to their once having possessed hearing, but the increase is so slight as not to warrant any dogmatic statement. At age five an increase of 4 points over age four takes place. From this point on (with the exception of age seven represented by only one case), there is a very decided but irregular and fluctuating increase in the medians. These results might lead us to some such tentative generalization as follows: Children who lose their hearing before the age of four or five are very little, if at all, benefited, as far as language ability is concerned, by having once possessed hearing. Further, every year of hearing after the age of four seems to increase the chances for a better acquisition of language. By language we mean here the ability to understand and compose. Our data has nothing to offer in regard to the ability to speak, read the lips or communicate by means of signs and the manual alphabet. The higher medians for those who become deaf in later childhood are reflected in the much higher grade ability as contrasted with hearing children. It will be remembered that the deaf as a group never reach fifth-grade ability. In the last column of Table VI. it will be noted that some of the groups go beyond fourth-grade ability, one reaching the level of eleventh-grade ability.

Comparison of Hearing and Deaf.—To bring out further the contrast between the hearing and the deaf in this test, we have constructed Table VII. This shows the number

TABLE VII

DISTRIBUTION ACCORDING TO HEARING GRADE ABILITY

Grade	Kentucky						Ohio						Ohio and Ky.	
	Oral		Manual		All Deaf		Oral		Manual		All Deaf		All Deaf	
	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%	No.	%
−2	34	27.4	6	6.5	40	18.5	20	10.7	24	20.9	44	13.4	84	15.5
2	21	16.9	26	28.3	47	21.8	42	22.4	45	39.1	88	26.8	135	24.8
3	42	33.8	47	51.1	89	41.2	61	32.6	36	31.3	111	33.8	200	36.8
4	20	16.1	12	13.0	32	14.8	46	24.6	8	7.0	59	18.0	91	16.7
5	1	0.8	1	1.1	2	0.9	8	4.3	1	0.9	12	3.7	14	2.6
6	2	1.6	...	.....	2	0.9	3	1.6	1	0.9	5	1.5	7	1.3
7	2	1.6	...	.....	2	0.9	3	1.6	...	.....	3	0.9	5	0.9
8	1	0.8	...	.....	1	0.5	2	1.1	...	.....	2	0.6	3	0.6
9	1	0.8	...	.....	1	0.5	1	0.5	...	.....	1	0.3	2	0.4
10	...	....	...	.....	...	.....	...	....	...	.....	...	....	...	.....
11	...	....	...	.....	...	.....	...	....	...	.....	...	....	...	.....
12	...	....	...	.....	...	.....	1	0.5	...	.....	1	0.3	1	0.2
College Graduates....	...	....	...	.....	...	.....	...	....	...	.....	2	0.6	2	0.4
Total..........	124	99.6	92	100.0	216	100.0	187	99.6	115	100.1	328	99.9	544	100.2

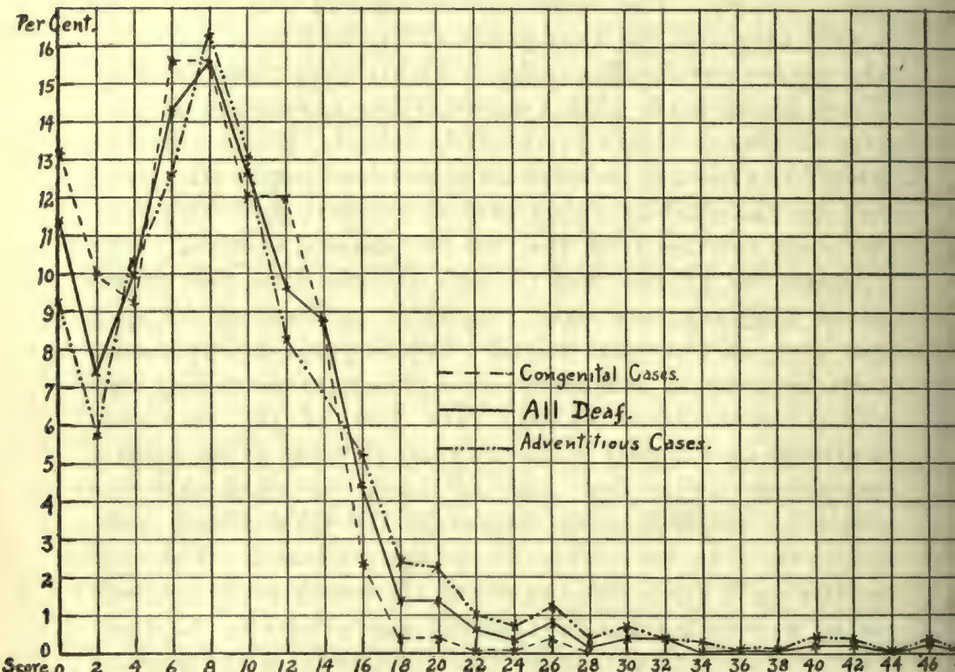
and per cent. of pupils having scores equivalent to each grade ability of hearing children. All those having a score less than 3 were counted as −2 or below second-grade ability. The median for grade 2 for hearing children is 4, and we have included in second-grade ability all those having scores from 3.1 to 6. The third-grade median is 8 and we have counted as third-grade ability all cases having scores from 7 to 11. In other words the grade ability was taken to include all scores lying between the score midway between the grade in question and the grade below up to the score lying midway between the grade in question and the grade above. The table shows the results for Ohio, for Kentucky and for both schools combined. Further the division into oral and manual pupils is shown. In this table the results for the second year's work were omitted, since only the second-year pupils in one school were tested. The inclusion of this group in one school alone would have spoiled a fair comparison by means of percentages.

In both schools the percentage of pupils in the oral classes who reach a high grade ability is much greater than in the manual classes. No manual pupil in Kentucky goes beyond

fifth-grade ability, in Ohio beyond sixth-grade ability. One oral pupil in Kentucky reaches ninth-grade ability, in Ohio twelfth-grade ability. Comparing the percentages in the "All Deaf" columns for the two schools we note a somewhat larger percentage in Kentucky making a score below second-grade ability. The largest percentage at any one grade ability occurs at the third-grade ability in both schools, but the percentage for Kentucky is higher than that for Ohio. From fourth-grade ability onwards the percentage at each step is always higher in the Ohio school. Under the "All Deaf" in Ohio are included 26 high-school pupils who could not be classified as either oral or manual, and who were therefore omitted from the oral and manual columns. The columns for all the deaf of both schools show that third-grade ability occurs most frequently, accounting for 36.8 per cent. of the total tested. Second-grade ability comes next in order of frequency; then fourth-grade ability and minus second-grade ability. The rest of the cases are scattered over higher grade abilities, showing at no point a percentage higher than 2.6. It should be noted that two pupils in the high-school grades in the Ohio school make scores equal to the median for college graduates. The total scattering in grade abilities above the fourth grade amounts in all to only 6.4 per cent. of the pupils tested. All these facts serve but to emphasize what we have said above about the poverty of language exhibited by the deaf child.

Distribution of Scores.—The percentage distribution of the scores made by the pupils is shown on Graph III. The base line represents the scores. The first point on the line shows the per cent. making a score of zero. At the point marked 2 all making a score of 1 or 2 are shown, at the point 4 all making a score of 3 or 4, and so on, each point along the base line including two points of the score. The solid line shows the results for all the cases. A score of zero is made by 11.4 per cent. of the cases. There is then a drop in the number making a score of 1 or 2. The curve rises steadily to a percentage of 15.6, making a score of 7 or 8. This is the most common of any one score made by the pupils. From

this point the percentage of pupils drops steadily to a score of 17 or 18, which is attained by a percentage of 1.4. After this point the percentage of cases at any one point of the curve never rises above one per cent. The broken line curve shows the percentages for the congenital cases and the dotted



GRAPH III. Distribution of Scores of the Congenitally Deaf, Adventitiously Deaf, and All Deaf

and broken line curve for the adventitious cases. It will be seen at once that the curve for the congenitals shows a greater number of cases at the lower scores, while the curve for the adventitious shows fewer cases than the congenitals at the lower scores and a greater number at the higher scores. Indeed the congenital curve stops at the point 25–26, whereas the adventitious shows a scattering of cases up to the 45–46 point. This is, of course, to be expected in view of our previous analysis of the performance of the two groups. However, it should be noted that there are cases of congenital pupils who make fairly high scores, notably the one case at

the 25–26 point. The boy making this score is probably well above the average deaf pupil in mentality. His score is equal to seventh-grade ability. It is perfectly possible, therefore, for a congenitally deaf child to attain a good command of language, but we are led to believe from the results as a whole that this will only be the case with pupils possessing exceptional mentality and a favorable environment. All the other high scores have been made by adventitious cases, and these are the cases where deafness occurred in later childhood.

Relative Difficulty of Each Sentence.—The question as to whether the language development of the deaf proceeds along the same lines as that of the hearing can be approached by a study of the relative difficulty of each sentence on the Trabue Scale. This scale represents sentences of progressive difficulty for hearing children. Is the relative difficulty of these sentences the same for the deaf? To answer this question the score made on each sentence by the deaf pupils tested was calculated. In this computation the congenital cases were kept in one group and the adventitious cases in groups according to the time at which deafness occurred. This was done in order to see whether there was any difference in the relative difficulty of the sentences for each of these groups. It was thought possible that the language development of the congenital pupils might proceed along slightly different lines from the language development of the adventitious. These considerations, therefore, resolve themselves into a comparison of the relative difficulty of the sentences for the deaf and the hearing, and further for the different groups of deaf among themselves.

Table VIII. shows the score made by each group of deaf children on each sentence of the scale. These are the actual scores. At the bottom of the table is given the number of children in each group and the perfect score for that group, *i. e.*, the score that would have been made by the group if every child had completed every sentence perfectly.

Table IX. shows the same facts expressed in percentages. The figures in this table show the per cent. that the score made is of the perfect score, and this enables us to make the

Table VIII

Scores Made by Each Group of Deaf Children on Each Sentence of Language Scale *A*

Sentence Number	Congenital	Unknown	Acquired?	0–1	1	2	3	4	5	6	7–15
2x	338	22	32	58	130	94	46	30	18	20	26
2y	336	26	35	57	123	102	52	36	18	20	28
3x	261	16	22	43	87	52	36	28	18	17	25
3y	240	21	26	44	101	66	34	22	16	18	26
4x	68	8	6	14	30	20	20	20	10	12	18
4y	272	18	26	47	96	78	46	30	13	16	24
5x	89	5	12	16	34	20	13	16	10	8	20
5y	60	7	5	11	17	21	17	8	10	8	20
6x	13	2	3	2	4	3	7	9	2	8	13
6y	7	....	2	4	2	7	5	6	...	6	16
7x	2	2	....	...	6	5	6	5	3	7	12
7y	12	2	....	1	14	5	8	5	1	7	15
8x	6	3	1	3	4	9	6	7	1	4	12
8y	3	....	1	1	3	...	5	5	...	7	10
9x	....	....	....	...	...	...	2	2	...	3	4
9y	1	....	....	...	...	...	2	1	...	2	4
10x	....	....	....	...	3	...	2	1	...	3	6
10y	....	....	....	...	...	...	...	4	...	2	2
11x	....	....	....	...	...	...	1	...	...	2	2
11y	....	....	....	...	1	...	1	...	...	2	1
12x	....	....	....	...	...	...	2	...	...	2	1
12y	....	....	....	...	...	...	1	...	...	2	...
13x	....	....	....	...	...	...	...	...	...	2	...
13y	....	....	....	...	...	...	1	...	...	...	...
No. tested.........	250	19	20	45	85	65	32	19	11	10	14
Perfect score......	500	38	40	90	170	130	64	38	22	20	28

comparisons in regard to the relative difficulty of the sentences. Mere inspection of this table shows that the pairs of sentences are roughly equally difficult for the deaf as they have proved to be for hearing children. The only striking exception to this is the pair 4*x*, 4*y*. For the deaf 4*x* is for all groups much more difficult than 4*y*. The difference between these two sentences might be described as a difference between an abstract and a concrete sentence. Sentence 4*y* is: "Boys and ...... soon become ...... and women." Sentence 4*x* is: "Time ...... often more valuable ...... money." The latter may be called more abstract than the former. The latter is relatively more difficult for the deaf than it is for the hearing. Furthermore, sentence 4*x* is somewhat in the nature of a proverb, and is probably heard by the hearing child a great many times in ordinary conversation. We see further from this table that as we proceed from the congenital

group up through the adventitious groups, the percentages for each sentence tend to increase steadily though not uniformly from group to group. This again bears out what we have noted previously as to the superior language ability of

TABLE IX

PER CENT. SCORE MADE BY EACH GROUP OF DEAF CHILDREN IS OF PERFECT SCORE FOR EACH SENTENCE OF LANGUAGE SCALE *A*

Sentence Number	Congenital	Unknown	Acquired ?	0–1	1	2	3	4	5	6	7–15	Total	Average Per Cent.
2x	67.5	57.9	80.2	64.5	76.5	72.2	72.0	79.0	81.7	100.0	92.9	844.4	76.76
2y	67.2	68.5	87.5	63.2	72.4	78.5	81.3	94.8	81.7	100.0	100.0	895.1	81.64
3x	52.2	42.1	55.0	47.8	51.2	40.0	56.4	73.8	81.7	85.0	89.3	674.5	61.31
3y	48.0	55.2	65.0	48.8	59.4	50.7	53.2	57.8	72.7	90.0	92.9	693.7	63.06
4x	13.6	21.1	15.0	15.6	17.6	15.4	31.3	52.6	45.4	60.0	64.3	351.9	31.99
4y	54.4	47.3	65.0	52.2	56.5	60.0	72.0	79.0	59.0	80.0	85.7	711.1	64.64
5x	17.8	13.2	30.0	17.7	20.0	15.4	20.3	42.1	45.4	40.0	71.5	333.4	30.30
5y	12.0	18.4	12.5	12.2	10.0	16.1	26.5	21.0	45.4	40.0	71.5	285.6	25.96
6x	2.6	5.3	7.5	2.2	2.4	2.3	10.9	23.6	9.1	40.0	46.5	152.4	13.85
6y	1.4	.....	5.0	4.4	1.2	5.4	7.8	15.8	....	30.0	57.1	128.1	11.64
7x	0.4	5.3	......	....	3.5	3.8	9.4	13.2	13.6	35.0	42.9	131.1	11.91
7y	2.4	5.3	......	1.1	8.2	3.8	12.5	13.2	4.5	35.0	53.6	139.6	12.69
8x	1.2	7.9	2.5	3.3	2.4	6.9	9.4	18.4	4.5	20.0	42.9	119.4	10.85
8y	0.6	.....	2.5	1.1	1.8	....	7.8	13.2	....	35.0	35.7	97.7	8.88
9x	......	.....	......	....	....	....	3.1	5.3	....	15.0	14.3	37.7	3.42
9y	0.2	.....	......	....	....	....	3.1	2.6	....	10.0	14.3	30.2	2.74
10x	......	.....	......	....	1.8	....	3.1	2.6	....	15.0	21.4	43.9	3.99
10y	......	.....	......	....	....	....	....	10.5	....	10.0	7.1	27.6	2.50
11x	......	.....	......	....	....	....	1.6	....	....	10.0	7.1	18.7	1.70
11y	......	.....	......	....	0.6	....	1.6	....	....	10.0	3.6	15.8	1.43
12x	......	.....	......	....	....	....	3.1	....	....	10.0	3.6	16.7	1.51
12y	......	.....	......	....	....	....	1.6	....	....	10.0	.....	11.6	1.05
13x	......	.....	......	....	....	....	....	....	....	10.0	.....	10.0	0.90
13y	......	.....	......	....	....	....	1.6	....	....	.....	.....	1.6	0.14

the adventitious groups and its relation to the age at which deafness occurred.

From Table IX. intercorrelations between each group and every other group were calculated. For this purpose the rank in order of difficulty for the sentences was used, and the correlations calculated by the Spearman formula,

$$R = I - \frac{6\Sigma g}{n^2 - 1}.$$

In all there were 55 correlations, since the adventitious cases acquiring deafness from age seven to fifteen were included in one group. The 55 correlations ranged from $r = .89$ to

$r = .99$. The median of the 55 correlations is .95 and the average deviation from the median is $\pm .017$. These correlations answer very definitely our question as to the relative difficulty of the sentences for the various groups of children. On the whole the difficulty is almost the same for the congenital as it is for any group of the adventitious. Evidently, then, the language development of these different groups proceeds along the same lines.

Correlations between the relative difficulty of the sentences for the hearing children tested by Trabue and the different groups of deaf were also computed in the same manner as the intercorrelations described above. The results are as follows, being in every case a correlation between the hearing children and the group of deaf as noted below:

Deaf Groups	r
Congenital Cases	.95
Unknown "	.91
Acquired?	.92
Before one year of age	.93
Between 1 and 2 years	.92
" 2 " 3 "	.93
" 3 " 4 "	.96
" 4 " 5 "	.97
" 5 " 6 "	.91
" 6 " 7 "	.98
From 7 to 15 years	.99
All deaf and all hearing	.99

These uniformly high correlations lead us to the conclusion that the difficulty of the sentences for the deaf is about the same as for the hearing and that, therefore, as far as can be determined from this test, the language development of the deaf proceeds essentially along the same lines as the language development of the hearing. This corroborates previous work by the writers in which the Woodworth and Wells Directions Tests were used.[1]

Correlation with Substitution Tests.—In one of the schools tested the Digit-Symbol and Symbol-Digit tests were given at the same time as the Trabue Language Scale and correlations between these tests were computed by the Spearman

[1] Pintner, R. and Paterson, D. G., *op. cit.*

R method. The first set of correlations was made between the ranks of the 21 classes in the three tests as determined in each case by the median performance. The coefficients are as follows:

Digit-Symbol and Symbol-Digit with Trabue . . . . $r = .92$
Digit-Symbol with Trabue . . . . $r = .93$
Symbol-Digit with Trabue . . . . $r = .90$
Digit-Symbol with Symbol-Digit . . . . $r = .96$

These high correlations show that the classes with the high scores in the substitution tests are the best in the language tests, indicating that the tests are good measures for groups of individuals.

The coefficients of correlation between the average rank of pupils in the Digit-Symbol and the Symbol-Digit tests and their ranks in the Trabue Language Scale by years of work are as follows:

Year of Work	Number of Pupils	*R*	*r*	P. E.
12	16	−.23	−.35	?
10	8	.38	.56	?
9	13	.31	.47	?
8	39	.32	.48	.097
7	27	.31	.47	.113
6	39	.15	.23	.110
5	11	.25	.38	.167
4	35	.19	.29	.110
3	25	.37	.55	.101
2	26	.42	.61	.086
		Average	.35	

The correlation between individuals at the same stage of instruction is much lower than the correlations of the class medians. In all cases, except one, the coefficients are positive, but the number of cases in some of the groups is very small. Again the P.E. is sometimes very large. On the whole there seems to be some relationship between the abilities tested but not a very close one.

Summary

1. The Trabue Language Scale has proved to be an admirable test for deaf children. The form of the test is sufficiently familiar to them to make the test easy to give.

Too much does not depend upon comprehension of instructions.

2. On the whole the pupils in the oral classes do better than pupils in the manual classes.

3. The grading of classes in both institutions as far as language work is concerned is poor. There seem to be no definite standards in language work for each class, nor even for each year of work.

4. Division of classes into sections according to ability as measured by a combination of mental and educational tests would increase efficiency in administration and lead to an economy of effort in teaching.

5. Tentative norms of language ability for oral and manual pupils classified according to year of instruction have been determined. These are given in Table III. Norms for all deaf are given in Table IV.

6. The average language ability of the adventitiously deaf is slightly superior to that of the congenitally deaf.

7. The adventitiously deaf who lose their hearing before age four or five do not seem to be superior in language ability to the congenitally deaf.

8. The adventitiously deaf who lose their hearing after age four or five seem to benefit so far as language ability is concerned by reason of having once possessed hearing.

9. The grade abilities of the majority of deaf children fall between −2 and 4. Very few deaf children (6.4 per cent.) reach scores above fourth-grade ability.

10. The language development of the adventitiously and the congenitally deaf proceeds along the same general lines.

11. The language development of hearing and deaf children proceeds in general along the same lines.

INDIVIDUAL AND SEX DIFFERENCES BROUGHT OUT BY FASTING

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During the first three weeks of July, 1915, the writer and his wife undertook to share the hardships of a lengthy fast. Though not of such rigor as endured by numerous predecessors, it was yet of such reality as to produce some distinct experiences; but unfortunately the most interesting and dramatic of these were not fixable by tabulation and graphometry.

General Routine

During the first of the three weeks the food was reduced gradually from normal to near-nothing; no food was taken during the second week, but from 500 to 750 c.c. of water daily; the food conditions of the first week were exactly reversed the third week. Accurate tests of the various activities were made in the psychological laboratory of the College of the City of New York from 9 A. M. to 1 P. M. daily. In addition, six days of "control" tests were made at odd times but, serving badly in that capacity, have been entirely omitted from treatment here. Observations of general physical welfare and of specific physiological facts, and introspections of general mental status and particular mental tests, were recorded diurnally. Accurate analyses of the blood by a skilled physician[1] and measurements of blood pressure, body temperature, pulse rate, weight changes, and lung capacity were made regularly. Graphic records of the barometric and thermometric conditions throughout the day, and verbal records of visible weather features four times a day, were taken. Our daily program provided also for customary occupations, afternoons and evenings, and was fairly uniform and controlled for the whole period.

[1] Dr. W. H. Boese, district clinical supervisor, Chelsea District of the New York City Health Department, etc.

The Tests

The series of tests employed covered what may roughly be described as weight and strength, rate and accuracy, sensitivity and passivity, memory span and retentivity. Group A includes (1) weight (stripped), (2) lung capacity (wet spirometer), (3) grip, right and left hands (Narragansett dynamometer, 5 trials each hand) and (4) fatigue (hanging by the arms to the utmost endurance).

Groups B and D include five tests, each measured by the time used and errors made: (1) Association (50 words daily from the Woodworth and Wells series, the qua ity of the associations being estimated by the number of low-grade reactions occurring); (2) naming (the 100 colors and the 100 forms of the W. W. blanks); (3) continuous addition (100 additions daily, W. W. forms); (4) continuous subtraction (100 subtractions daily, W. W. forms); and (5) mental multiplication (ten 2-place problems each day).

Group C includes (1) Touch (single camel's hair, applied 10 times each to top and bottom of tip of nose, positive and negative results being recorded as the hair was or was not sensed); (2) pain (10 threshold values secured from tip of nose by a Verdin algometer); (3) sight (perception of dots, irregularly arranged and ranging from 4 to 9 per card, 30 cards in all, exposed with drop screen); (4) steadiness (tracing a gradually narrowing slit, 25 cm. long, with a metal stylus, contacts with the sides being registered by an electric buzzer, 10 trials with each hand). Taste and smell were tested for two weeks and then dropped, due to the delays and difficulties they entailed; but the results would have proven as interesting as any, had this section of our plan been completed.

Group E includes (1) Immediate memory span (10 minutes allowed for memorizing the 50 words of the association list of the same day); (2) Reproduction (10 minutes allowed for recall of as many words as possible from the preceding day's memory list). The whole series of tests, it will be seen, is fairly comprehensive.

Method

After the results were secured in terms of quantity (or time) and quality (or mistakes), they were reduced to relative ranks in each test. That is, they were distributed into ten classes giving the most normal scale. By this method the value of the steps between successive ranks varies, of course, from test to test, but in each test remains the same for both sexes, with the necessary change in starting point. It is obvious that a change amounting to one step in rank equals a change of 10 per cent. of the total range of variation displayed in each case.

An example will make this plain. For 20 days in subtraction, the times for the male in seconds were 480, 438, 398, 351, 391, 355, 369, 308, 351, 330, 349, 330, 310, 312, 296, 288, 304, 288, 258, 240; and for the female 500, 498, 431, 416, 366, 357, 329, 330, 344, 310, 285, 300, 285, 296, 292, 298, 252, 250, 255, 220. By inspection and trial it is found that the best interval to use as step is 30 seconds, and the best scale of ranks is produced by beginning for the male at 220 and for the female at 230. The tenth ranks then are '460-up' and '470-up,' respectively. Likewise the errors, ranging from none to six in the one case, and from none to nine in the other, were ranked with the step-value equaling one half.

By this method extreme cases at either end of the series are considerably 'smoothed'; but this may be an advantage rather than a disadvantage, because in a measure it counteracts the influence of uncorrected practice effect. It will be noticed that the lowest score practically is counted zero and the best, and that the rest are related to it in successively *worse* classes up to ten. Therefore in the last table, the results are in grades of *deficiency*, the larger the number the worse the ability. The value of this method of ranking, in the present investigation, lies in its provision for easy comparison of the individual with his own record rather than with an external standard; and of keeping all the results alike in form and hence in a certain sense directly correlatable in the different tests and for the different persons. Estimate of the reliability of the figures shown in terms of the prob-

able error was not made, due to the small number of subjects involved.

Table I

Air and Blood Changes

Week and Sex	Air		Blood			Hemoglobin	Common Cells		Differential Count				
	Temp.	Press.	Pulse	Temp.	Press.		White	Red	P., Per Cent.	L., Per Cent.	M., Per Cent.	E., Per Cent.	B., Per Cent.
1st, M......	85.3	29.81	69.3	98.1	30	90–100	7,700	5,980,000	45.0	44.0	3.5	7.0	0.5
F.......	85.3	29.81	66.9	97.8	36	" "	7,100	5,190,000	56.0	36.0	3.0	4.0	1.0
2d, M......	82.7	29.85	71.4	97.8	14	" "	6,200	6,650,000	67.0	26.0	5.0	2.0	0.0
F.......	82.7	29.85	61.9	97.5	18	" "	6,460	6,440,000	48.0	41.0	9.0	1.0	1.0
3d, M......	82.3	29.95	75.6	98.4	33	" "	6,200	5,960,000	46.5	41.0	3.5	7.5	1.5
F.......	82.3	29.95	69.3	97.7	40	" "	7,920	5,890,000	47.8	43.9	5.5	2.5	0.3

P. = polynuclear; L. = lymphocytes; M. = mononuclear; E. = eosinophiles; B. = basophiles.

Results

The results of least psychologic interest are given first and condensedly, mainly as collated in Table I. The 'air' figures were secured from a seven-day Tycos baro-thermograph, and show that the average conditions were unusually constant; hence any marked mental influences from this quarter could be ascertained only by careful correlations for each day.

The 'blood' figures are more significant, the fall in pressure for the fasting week being especially pronounced for both sexes. Temperature and pulse also fall, the only sex difference here appearing in the more marked change for the female in heart rate. It is possible that the cellular changes in the blood are most significant, though a glance suffices to show that this is not true of the white and red corpuscles. The latter fact serves to cast doubt on the popularly accepted view that the organism radically is weakened by fasting (a week or ten days)—that the guardian white cells are depleted and thus leave the body exposed to the ravages of bacterial diseases. The writer will not here try to interpret the other cellular changes tabulated, but has included them for the benefit of those who may be interested.

The results of greatest psychologic interest are sum-

marized in Table II., classified roughly under five headings for readier handling, and described briefly as to the outcome of each group. The column marked 'descending' covers results for the first week, the first day excluded due to unavoidable irregularities; the one marked 'fasting' covers the second week and first day of the third week, the latter included because the fasting momentum was unquestionably stronger than the food influence; and the one marked 'ascending' covers the last six days of the period.

By 'fasting loss' is meant that decisive debility was produced by the food deficit; by 'practice loss,' that the anticipated gain due to practice was slowed in rate or not fully realized; by 'practice gain,' that the expectable practice gain was not impeded but appeared in normal degree; and by 'fasting gain,' that decisive ability was produced by the food absence.

Not only does the larger number mean the less ability, but the table is arranged to show first in order, both in the whole table and in its parts, those activities rendered most defective by the fasting.

The first point of interest is the high correlation between the estimated food intake and the changes in weight and strength. The results are practically uniform in trend and are in the direction one would expect.

The next thing of note is that the general trend of the next group, though indicated as "practice loss" is much less decisive than in the preceding case. This partly depends on the fact that the group is mixed, the first three tests involving an important motor element and the last two not. That is, the first three tests tend to follow the preceding group in fasting loss while the last two, more purely mental in character, more nearly simulate the succeeding groups.

In the next group, *C*, the trend is no more distinct. This again is a mixed group but does on the whole show pretty complete practice gain. There are here, as in the other groups, certain sex differences brought out. Sensitivity in the female apparently decreased in touch and sight and increased in pain in the full-fast period, but the reverse was true for the male. Otherwise sex similarity prevails.

TABLE II

SUMMARIZED RESULTS OF FASTING TESTS

[The larger the number the less the ability]

Activities Tested	Descending		Fasting		Ascending		Group Names and Results (Estimated)
	M.	F.	M.	F.	M.	F.	
Food Intake	4.5	4.5	8.5	8.5	3.5	3.5	
Weight Loss	7.3	7.2	7.9	7.0	3.9	4.0	
Lung Capac.	5.3	2.8	6.8	7.5	2.8	4.5	
Grip—Right	2.3	5.7	6.1	6.9	6.8	5.1	*A.* Vitality
Grip—Left	4.0	5.7	7.7	5.7	7.5	5.1	
Fatigue	7.3	4.3	8.2	8.4	1.3	3.7	
	5.2	5.1	7.3	7.1	4.5	4.5	Fasting loss
Association time	4.3	5.2	7.3	5.0	4.2	3.3	
Naming time	4.1	6.0	3.6	6.7	1.8	3.6	
Addition time	6.3	6.0	6.0	6.9	4.5	2.8	*B.* Rapidity
Subtraction time	7.5	8.7	5.4	5.1	3.4	3.3	
Multiplication time	8.7	8.8	6.3	6.0	2.5	1.8	
	6.2	6.9	5.7	5.9	3.3	3.0	Practice loss
Touch	6.4	5.7	5.1	6.2	4.8	3.9	
Pain	4.0	6.5	6.0	5.0	5.5	6.8	
Sight	6.3	6.6	3.8	4.7	5.5	3.5	
Steadiness time—R	6.5	4.8	2.7	4.0	5.3	5.7	*C.* Passivity
" " —L	4.0	4.0	5.5	4.6	5.2	6.2	
" space—R	6.3	5.0	3.6	5.4	3.2	3.5	
" " —L	7.3	7.3	4.6	4.4	2.7	2.5	
	5.8	5.7	4.5	4.9	4.6	4.6	Practice gain
Association errors	6.5	5.5	7.0	7.0	4.7	6.0	
Naming errors	5.5	6.1	4.1	1.8	2.9	3.3	
Addition errors	4.0	6.0	2.5	4.0	3.5	1.0	*D.* Accuracy
Subtraction errors	8.3	7.8	3.5	3.3	4.5	3.8	
Multiplication errors	5.3	7.2	5.1	6.0	5.7	5.5	
	5.9	6.5	4.4	4.4	4.2	3.9	Fasting gain
Memorization	4.0	4.7	8.1	2.7	4.8	6.5	*E.* Memory
Reproduction	5.0	4.7	8.5	4.2	5.0	8.1	
	4.5	4.7	8.3	3.5	4.9	7.3	M., loss, F., gain
Total	5.6	5.8	5.7	5.3	4.2	4.1	

In group *D* it must be admitted that the results are not very homogeneous in their showing or meaning. We have here that part of the given tests most distinctly mental, and while they show most betterment or fasting gain from the food abstinence, they also show most variability and hence

unreliability. As for sex, it may be seen that there is almost perfect agreement in the several traits instead of any differences.

In truth it is only in the last group, *E*, apart from the instances already mentioned, that an undoubted difference appears, the male showing excessive deficiency and the female excessive proficiency from the given ordeal. Even here, of course, it cannot surely be said to be a 'sex' affair, since any two individuals might exhibit the same fact.

Though true that the test-results are technically most interesting, it yet is true that the introspections are informative. I shall mention some things they show of a general nature or about certain days or tests and in relation to shifts in energy, feeling, and intellection.

First, concerning the amount of correlation between the objective and subjective effects. The food was decreasing from the start and quite absent from the 7th to the 15th, yet it may be judged that any effect from its absence would increase after the 7th to a climax, due to new adjustments entailed on the organism; and then would decrease, due to their establishment and to the exhilaration of the approaching end. This climax does come about the 11th, showing in introspections, performances, and weight. Thus the male losses in weight from the 9th to the 12th are nearly nine pounds, from a total of nineteen; the female, five and a half, from a total of fourteen. In the tests, after a starting spurt of high efficiency on the 8th, there was a reactionary drop on the 9th and thence to the 12th, from which time improvement proceeded irregularly to the end.

Introspective records about the date of this turning-point were on the following order. Male: "Heart pounds greatly and more or less constantly, particularly at the pit of the stomach; also beats rapidly and flutters at stair-climbing. Unable to do work afternoon or evening. Great lassitude and discomfort of general nature and in head. Throat dry but no desire for water nor liking of it. Pains in head, eyes, back, legs, especially knees and calves—lying, sitting, or standing. No nausea, but sense of instability in the stomach

—of being easily upset. Feelings of sympathy, joy, reverence, etc., all are reduced in quality—lifeless in fact." Female: "Bath gave no pleasure. Felt like bursting out crying during mental tests and in afternoon. Greatly startled by sudden noises. Experienced hunger for the first time. At night dreamed of fried cucumbers and chopped olives being used as padding around some mechanical apparatus. To undertake anything seems a great effort and I can't speed up beyond a jog trot. My emotions have returned (on the 12th) after several days of torpor." Though physical fatigue and inertia continue after the 13th, mental conditions begin to mend, as already suggested.

The question of 'how it feels to fast' is of main concern to most persons who have not indulged in severe dietary regimens. While the characteristic hunger sensations were experienced at meal times on several days, even occasionally accompanied by belchings, nausea and vomiting, especially in the case of the female, yet the discomfort on this account is not as gigantic as it looms in imagination. No doubt it would be truest to say that it is masked by the prevailing somatic state of ill-being or odd-being, relativity being so great a factor in all experience. The complexity and intensity of this state would appear were our introspective terms fully listed here, instead of merely sampled in the preceding paragraph.

Concerning the correlation of specific introspected feelings and actual test results, our records show very high agreement at times and equally low at others. Of 100 judgments recorded by the male subject, concerning his feelings of progress and accuracy in particular tests, 60 showed positive correlation and 40 negative. For the female the matter is more ambiguous and quite undecipherable mathematically, as this typical quotation will show: "I feel as though I am very slow in doing my tests even though they are shorter in time than usual. I have a feeling of impatience as though one part of me were having to wait for another part; and it is almost as though the impatient part were saying 'Stop trying and let me do your work; I can do it faster and better than you': and when I do stop trying, this unseen part puts

down the answers with such great ease and speed that I marvel at its achievements." This dual-personality experience was present a number of times and the side of it which felt like 'doing things' often produced results just the opposite. In fact the correlation between the work as appraised subjectively and as measured objectively seems for the female about as negative as for the male it was positive. But the distribution of judgments in both cases would be rather close to that of 'chance.'

Conclusion

The immediate effects of the fast as shown by the tests indicate a depletion of vitality and strength commensurate with the reduction of food intake; and to some extent a slowing-down in the speed of activities, more for the motor than the mental. The sensory and passive sides of the self are not greatly affected, generally speaking, but sexually show male sensitivity for pain and perceptivity for dots increased, and for touch decreased; while for the female the reverse is true. Some improvement for both sexes is shown in mental clearness and accuracy, though not decisively; and a most pronounced effect upon memory, disadvantageous for the masculine subject and advantageous for the feminine. The feelings, unusually acute for several days and then unusually apathetic for a time, were on the whole ambiguous indices of the grades of objective performance, less so for the male than the female. Had the efforts required by the tests been more protracted and exacting in character probably the subjective-objective correlation would have been higher.

The ultimate effects of fasting, upon both health and ability, it is most desirable to know. Doctors often do not promise radical health changes without months of definite dietary changes. Ancient reports of fasts are bad scientifically through lack of proper control and measurement. Modern researches cover these points better but are bad practically from failure to shed light on an after-period. The present experiment fails similarly, due to a shift from laboratory to camping life concurrent with its completion. Instead of remaining a negligible part these remoter effects should be made the main goal of future fasting studies.

THE HUMAN SALIVARY REFLEX AND ITS USE IN PSYCHOLOGY

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The experiments of Pawlow and his students upon the conditioned salivary reflex have provided a method of investigation, which already has proved useful in the study of the sensory physiology of animals, and which promises to be even more valuable in revealing fundamental factors of habit formation and of central inhibition and reinforcement. Pawlow's technique is restricted rather narrowly, however, to larger mammals which combine a relatively abundant salivary flow with the possibility of operative methods; practically to dogs among laboratory animals. The restriction is not without advantages, since it tends to concentrate the work upon a single organism, but an extension of the method to man also seems desirable, as there is little agreement in the results obtained by the use of the conditioned salivary reflex and of motor habits.

Zeljony ('07) and Johnson ('14) have obtained quite conflicting results in studies of audition by the use of the two methods. In the field of vision, too, there are similar discrepancies which may or may not be due to the fact that the Russian investigators have not used well-controlled stimuli. A revision of the work with the possibility of a difference in the thresholds of muscular and glandular reactions is desirable. This can be carried out to best advantage with human subjects.

Pawlow has largely refrained from anthropomorphic interpretation of his results but some of his followers are not free from this error. Dontchef-Dezeuze ('14) especially, has given an elaborate and wholly unjustified interpretation of the process of association in conditioned secretion in terms of images and affective elements. In the present stage of

objective psychology her assumptions can be tested only by experiments on man. The whole problem of the relation of emotional reactions to the conditioned reflex is untouched and the stimuli exciting these can be most easily controlled with human subjects.

Finally, the increasing emphasis which glandular activity is receiving in physiological psychology calls insistently for a fuller understanding of the conditions governing secretion than we have at present. While the salivary glands are perhaps the least important of all the glands for psychology they offer the only opportunity for the direct study of the relation of stimulus and glandular reaction in the human subject. The remaining digestive glands, the lachrymals, the reproductive and ductless glands are too well protected for direct study and the sebaceous glands offer great difficulty because of their scant secretion. To what extent conclusions drawn from the salivary glands can be applied to other glandular reactions can probably be determined only by operative experiments on animals.

A number of studies of the human salivary reflex have appeared, but for the most part they deal with detached observations and no general summary of the work has been made. Such a summary seems desirable for orientation in further experiments and in the following pages an attempt has been made to review the more important papers dealing with different aspects of the reflex in man. The literature upon the salivary reflex in other mammals is very extensive, including more than two hundred studies, and it has not been possible to do more than mention the general results of this work as indicating points which must be considered in any experiments upon the conditioned salivary reflex in man. A number of papers dealing with variations in the chemical composition of the human saliva have been cited only where they indicate a differential reaction to stimulation. In general only such material has been included as seems to bear directly upon the reflex mechanism of secretion. In summarizing the literature it has seemed best to consider it in topical rather than in historical form, since the latter would necessarily involve much repetition.

Practically all existing studies of human salivary secretion have been confined to the parotid gland, which is most easily accessible. In animals, on the other hand, the submaxillary has been studied most extensively. The innervation of the two glands is, however, essentially the same in its general features, although derived from different nerve roots, and there is no evidence for any fundamental difference in their mechanism of reaction. They show differences in excitability to specific stimuli but their reflexes are alike in their general capacity for inhibition and modification by training.

Methods of Obtaining the Secretion

There has always been a considerable difficulty in isolating the secretion of the single glands, which has militated against long-continued studies of salivation in man. Three methods of obtaining the secretion have been used. Lassaigne and Tuczek ('76) had their subjects chew dry food and measured the increase of this in weight, thus obtaining the total secretion. The range of applicability of this method is obviously limited. Eckhard ('63) and Oehl ('64) have worked out a method of using a canula inserted into the ducts of the larger glands. According to Ordenstein ('60) the method is not accurate since leakage of the secretion around the canula may occur and it is exceedingly difficult to keep the canula in place. The technique of inserting the canula is somewhat difficult also, and Babkin ('14) considers that its presence in the duct leads to a continuous reflex excitation of the gland. The most thorough studies have been made upon subjects in whom a fistula of one of the salivary ducts has been established accidentally. Such cases, however, are rare, and have thus far been found to involve only Stenson's duct. The writer has devised a simple drainage tube to be attached over the mouth of Stenson's or of Wharton's duct, which is free from most of the objectionable features of the canula.[1]

Secretion of the Glands in the Absence of Stimulation

The existing data upon the normal rate of secretion of

[1] This apparatus has been figured by Watson ('16) and will be described in a paper to appear later in the *Journal of Experimental Psychology*.

the parotid gland in the absence of extero-stimulation shows a great amount of variation in the glandular activity of different individuals. Zebrowski ('05), using subjects with fistulas of Stenson's ducts, and Brunacci ('10) using a canula to drain off the secretion, find little or no glandular activity during rest. Butler-Stoney ('73) found a little secretion in a subject with a fistula but gave his data in terms of the time required to fill Stenson's duct so that his results are not comparable with those of others. The more accurate records given by other investigators are summarized in Table I. The results, with the exception of those of Küss, are based upon observations extending over several hours during which the secretion remained fairly uniform. The secretion found by Butler-Stoney, Mitscherlich, and Küss in subjects with fistulas indicates that the secretion obtained by the use of the canula is not, as Babkin ('14) thinks, wholly the result of excitation by the canula, but that there is a constant slight secretion of the parotid gland without extero-stimulation. This, however, may be reflex in nature, since there is probably a constant stimulation of the mucosa of the digestive tract.

TABLE I

SUMMARY OF THE EXISTING DATA UPON THE RATE OF SALIVARY SECRETION IN MAN, IN THE ABSENCE OF EXTERO-STIMULATION

Subject		Secretion Grs. per Hour	Conditions of Stimulation	Investigator	Method of Collecting Secretion
Sex	Age				
Parotid gland					
♂	40	0.08	Subject sleeping.	Mitscherlich ('33)..	Fistula
		0.53	Reading quietly.		
♂	17	11.80	Subject quiet: Vegetable diet.	Ordenstein ('60)...	Canula
♂	18	1.00	Mixed diet....		
♂	?	4.50	Meat diet.....		
♂	49	1.10	Vegetable diet.		
♂	60	0.80	Subject quiet......	Küss ('99)........	Fistula
♂?	?	1.70	Subject quiet....	Oehl ('64).........	Canula
♂?	?	2.20	" "		
♂?	?	1.66	" "		
Submaxillary gland					
♂?	?	7.12	Subject quiet....	Oehl ('64).........	Canula
♂?	?	6.00	" "		

Ordenstein ('60) alone has observed the rate of secretion in a human subject for a twenty-four-hour period and the

individual with whom he worked, a poorly nourished boy, was probably, as Buff ('88) suggests, pathological. Little variation appeared in the rate of secretion of this subject during the twenty-four hours except after food was taken, when an increase in the rate of secretion appeared.

Mitscherlich ('33) studied a man, forty years of age, with a fistula of the left Stenson's duct. He found that during sleep the gland produced about 0.09 c.c. of saliva per hour; during quiet reading with movements of the mouth from 0.41 to 0.53 c.c. per hour; during meals the quantity of secretion varied from 0.8 to 2.0 c.c. per minute. For ten hours during a fast when the subject was unusually quiet the saliva was collected constantly but not enough was obtained for measurement. In this case the amount of secretion seems to be proportional to the degree of activity of the subject.

In the observations of Ordenstein there was no correlation between the amount of secretion and the diet of the subjects studied. Tuczek found a relation between the total quantity of saliva secreted and the age and sex of his subjects, women and children giving less secretion than men. Since different individuals may vary in the size of the food particles which excite the swallowing reflex and in the relative efficiency of gustatory stimuli to excite secretion, the method of Tuczek, which required the subject to chew dry food until a bolus was formed ready for swallowing, is not reliable as a measure of individual difference.

The other human salivary glands have been studied scarcely at all. Eckhard ('63) described a method of obtaining the secretion of the submaxillary by inserting a canula into Wharton's duct and made some observations upon its chemical properties, but did not study the reflex excitability of the gland. Oehl ('64) determined the rate of secretion of the submaxillary gland in two normal human subjects to be 7.12 and 6.00 grams per hour. He also described a method of obtaining the secretion of the sublingual gland but made only a few observations on the chemical composition of the saliva obtained.

The Unconditioned Salivary Reflex

Reflex secretion is excited most readily, perhaps exclusively, by stimulation of the oral mucosa. Popielski ('09) criticizes the work of several investigators because they failed to control the temperature of gustatory stimuli, but offers no evidence to show that the temperature of food is an adequate stimulus to secretory activity. Brunacci ('10) tested the stimulating effect of water at temperatures ranging from 0 to 80 degrees C. applied to the oral mucosa and found that temperatures above 60 and below 15 degrees have a slight excitatory effect, calling out from one to three drops of secretion for each cubic centimeter of water placed in the mouth. He holds that the excitation here is mediated by the pain receptors rather than by those for temperature.

Mechanical stimulation of the mucosa of the mouth is believed by most investigators to be an effective stimulus to salivary secretion. Some evidence from animals seems to support this view, but it is not altogether conclusive. Heymann ('04) is cited by Babkin as having obtained secretion by mechanical stimulation of the tongue of the dog but so much of his data as is available shows very little excitability to mechanical stimulation uncombined with gustatory, or with chewing movements. Popielski obtained abundant secretion when he placed sand in the mouth of a dog and considered this reaction to be the result of the penetration of fine particles to the bases of the papillæ, where mechanical stimulation should be most effective.[1]

In man the evidence for excitation by tactile stimulation of the salivary glands is derived wholly from observations upon the effects of chewing. Zebrowski, in particular, has ascribed the abundant secretion obtained by chewing tasteless objects to tactile stimulation and Brunacci also has failed to distinguish between the mechanical and other stimuli involved in chewing. Ordenstein has shown secretory

[1] From experiments with human subjects I am inclined to think that the stimulus from sand is complex, involving primarily a reflex contraction of the throat with slight nausea which is an effective salivating agent in the absence of mechanical stimulation. A small amount of sand excites a much more abundant secretion than any amount of stimulation of the tongue and hard palate with a stiff-bristled brush.

reactions to stimulation of the walls of Stenson's duct but no other studies of the tactile excitability of the gland have been made.

The results obtained by various workers upon the effects of chewing are quite conflicting. Bernard ('55) states that, except in ruminants, the parotid functions only during chewing. Colin ('52) believed that chewing is an effective stimulus only because it reduces the size of the food particles and so allows a more ready stimulation of taste. Wulfson ('00) denies that chewing has any effect upon secretion. These results were obtained with animals, chiefly with the dog and horse. Küss gives evidence to show that in his subject chewing movements with empty mouth increased the rate of secretion from 0.8 to 20.0 c.c. per hour. Zebrowski states that chewing movements with empty mouth have no effect upon secretion, that chewing tasteless wax has little effect, and that bread is a much more effective stimulus if chewed than if held quietly in the mouth, thus supporting the view of Colin. Brunacci ('10) obtained from 7 to 15 drops of saliva per minute by chewing tasteless rubber and ascribes this, without further attempt at analysis, to mechanical stimulation.

The lack of agreement in these observations suggests that there must be a great deal of individual variation in the excitatory effects of chewing. My own experiments indicate that the reaction is fully as complex as many conditioned secretory reflexes and depends to a large extent upon previous visual and cutaneous stimuli.

Zebrowski found that chewing upon one side stimulated the gland of that side more than the other, thus confirming an earlier observation of Colin on the horse.

Swallowing movements have been found effective stimuli by Brunacci ('10). His subjects swallowed many times in rapid succession to produce secretion and the possible excitatory effects of movements of the tongue, etc., were not excluded.

No very extensive studies of the glandular reactions in man to gustatory stimuli have been recorded. Mitscherlich

('33) found a greater secretion in reaction to tasteful than to tasteless foods but could not carry out experiments with his subject. Tuczek gives data upon the amount of secretion obtained in chewing a large variety of foods, both moist and thoroughly dried, finding that secretion varies inversely as the water-content of the food and is influenced by the factor of taste. Zebrowski has made the most extensive study of the relation of the quantity of secretion to the amount of the stimulating chemical substance. In a large number of tests in which his subjects chewed and swallowed measured quantities of dry bread he found that the amounts of secretion were almostly exactly proportional to the square roots of the weights of the bread taken, and from these observations he deduced the law that the quantity of secretion is directly proportional to the square root of the intensity of stimulation. This does not agree with the observations of Popielski with the dog. He found in a few tests with water and sand that the quantity of secretion is directly proportional to the absolute quantity of the stimulating substance. Brunacci has pointed out the complexity of the stimuli involved in Zebrowski's experiment and considers that his law holds only for the particular case.

In all studies of the relation of the intensities of secretory reaction and stimulus very crude methods of applying stimulation have been used and it is not possible to say that the variations in the quantity of secretion resulted wholly from changes in the intensity of stimulation. The excitatory effect of a given stimulus varies with the area of the tongue and palate stimulated, with the duration of stimulation, with the rate of dilution of the stimulating substance with saliva, and probably with other variables besides the concentration of the stimulating substance. Thus Zebrowski's experiments are complicated by the different times required to reduce the different quantities of bread to a bolus ready for swallowing and by the rate of solution in the saliva of the gustatory substances from different quantities of bread.

Brunacci has shown that the secretion of the human parotid increases with increasing concentration of acid stimulus

solutions but has made observations with only a few concentrations. Little more than this has been discovered in the studies with animals.

In the experiments of Popielski it was shown that the secretion induced by iso-percentage solutions of acids is closely proportional to the molecular weights of the acids; in other words, that the excitatory effect of an acid stimulus is proportional to the degree of ionization of the acid. Experiments with other gustatory stimuli have not given any results capable of generalization beyond the facts that all gustatory stimuli excite secretion and each excites a specific quantitative reaction.

In this connection the qualitative changes in secretion after stimulation with different taste substances should be mentioned. This has been studied by Zebrowski and by Brunacci. The former found variations in the organic content of the parotid saliva ranging from an average of 0.10 per cent. with saturated salt solution to 1.48 per cent. with coagulated egg albumin. The alkalinity of the secretion was found to vary also, being greatest after stimulation with acid. The latter has made similar observations with a more extensive analysis of the physical and chemical properties of the secretion, including density, osmotic pressure, conductivity, and digestive power. There are specific qualitative reactions to gustatory stimuli.

In experiments with dogs a close correlation between the percentages of solid substances in the secretion elicited by gustatory stimulation with different foods and the conditioned secretion to the sight and smell of the same foods has been demonstrated by Sellheim ('04). Similar observations have not been made on man.

The excitability of the salivary glands in man to stimuli other than those applied to the mucosa of the mouth has not been demonstrated. The fact that during hunger the sight, smell, or thought of food excites secretion has been mentioned frequently but in attempts to obtain this secretion under conditions where it could be measured it has not appeared. There is good evidence that in the dog the secretion

following the sight or smell of food is an acquired reflex. Snarski ('01) found no reflex secretion in this animal to the sight of food. Zitowitsch ('11) fed young dogs exclusively on milk for half a year and found, at the end of this time, that secretion was excited by the sight and smell of only milk or milk products (cheese, etc.). Reflex secretion to gustatory stimulation by other foods appeared, however. Finally, Zeljony ('11–'12) found that ablation of the cortex in dogs abolished the salivary reflex to all stimuli except those applied directly to the oral mucosa. From these experiments it seems probable that in man also the reflex secretion at the sight and smell of foods is acquired and that the laboratory tests have not fulfilled all the conditions necessary for the appearance of the reflex.

A few other direct salivary reflexes have been described, but their position as unconditioned reflexes is doubtful. Aschenbrandt ('81) found that irritation of the cornea of the dog resulted in heightened salivary secretion and attempted to trace out a direct reflex path from the eye to the salivary glands. Buff ('88), who repeated his experiments, was unable to get any evidence of such a reflex and thought that the secretion observed by Aschenbrandt was due to the struggles of the animals. Oehl ('64) likewise failed to obtain any such reflex by stimulating the human cornea.

Jänicke ('78) advanced evidence for a reflex from stimulation of the mucosa of the stomach by chemicals introduced through a fistula. This was denied by Braun and by Buff, whose experiments showed no increase in secretion after such stimulation. Ordenstein's data indicate an increase in secretion after his subject took food but the after effects of the gustatory stimulation in eating were not controlled. The existence of a direct reflex from the stomach is not established.

Parfenow ('05–'06) showed that in the dog the secretion of saliva is increased when the temperature of the surrounding air is raised above 21 degrees R. The reflex nature of this secretion is established only indirectly by the fact that the body temperature of the animals did not rise during the experiments.

In explanation of the results of Aschenbrandt and of Jänicke, Buff assumed that active movements on the part of the animal lead to an increase in secretion, but he gave no experimental evidence in support of this opinion. His view is supported, however, by the observation of Tangl ('96) that horses which have had violent exercise show more fluid in the stomach than those which have remained quiet.

Colin ('54) states that salivation is increased during sexual excitement in animals and von Bechterew that light stimulation of the genitals increases the rate of secretion in the dog. Neither author, however, reports his observations in detail and in view of certain observations of my own upon the inhibition of the swallowing reflex during sexual excitement I am inclined to believe that the seeming increase in secretion observed by these authors was due merely to the retention of the normal secretion in the mouth.

The increase in secretion described in many types of paralysis needs further investigation. Drooling of saliva has been taken as an indication of this increase but no actual measurements of the flow have been made.

Inhibition of Secretion

Pawlow ('78) was the first to report an inhibition of the normal salivary secretion. He found that when he opened the body cavity of the dog and drew out the folds of the intestine there was a reduction in salivary secretion which persisted so long as the protopathic stimulation was continued. He considered that this inhibition was the result of the excitation of specific reflex paths from the region stimulated. Fubini ('94) obtained a similar inhibition of secretion when he violently stimulated the skin or the sciatic nerve of the dog. His observations suggest that there may be, perhaps, an inhibition from any protopathic stimulus and that Pawlow was not dealing with a restricted reflex.[1]

Brunacci and DeSanctis ('14) have reported experiments to show that intense mental application (translation from a

[1] My own observations on man have shown inhibition of salivary secretion following protopathic stimulation of the skin.

foreign language, computation, etc.) partially inhibits secretion of the parotid. Their method was to place a small quantity of dilute acid in the subject's mouth at the beginning of each successive minute during observation and record the secretion obtained as the result of this stimulation, applied while the subject was at rest and while engaged in mental work. A serious defect in this technique is the fact that the reaction to a gustatory stimulus varies with the area of the mouth stimulated and this is dependent largely upon movements of the tongue. When the subject is distracted the movements of the tongue are largely inhibited and the result is a lesser degree of stimulation. In my own experiments where this method was used an apparent inhibition of secretion sometimes disappeared with careful attention to the distribution of the stimulating substance. I can not find that Brunacci and DeSanctis have controlled this source of error.

A few cases of hysterical inhibition of secretion have been reported (Hadden, '89). In the patients described a complete absence of all salivary secretion appeared and persisted for months. Secretion could be obtained at first by the use of pilocarpin but the effect of the drug grew progressively less. Spontaneous recovery has been observed, but the condition has not been studied adequately.

Summary

These rather unrelated studies include practically all the material bearing upon the direct reflex in man. They show excitability of the glands to a large variety of stimuli applied to the oral mucosa but disagree in many important details. The chief unconditioned salivary reflexes are summarized in Table II. It will be seen from this that mechanical and gustatory stimuli and the withdrawal of water from the oral mucosa are the only excitants of salivary secretion, investigated by more than one man, concerning which there is agreement. With respect to gustatory stimuli there are no complete studies of the quantitative reactions to different qualities and intensities of stimulation yet the few observa-

Table II

Stimuli Exciting Reflex Secretion of the Salivary Glands in Man and Animals

+, Reflex present: −, Reflex absent

Stimulus	Reaction in Man	Investigator	Reaction in Animals	Investigator
Oral:				
Gustatory	+	All	+	All
Thermal	−	Brunacci	+	Heymann
Tactile	+	Zebrowski; Brunacci	+	Heymann
Withdrawal of water	+	Tuczek; Zebrowski	+	Popielski
Protopathic			+	Heymann
Organic:				
Chewing	+	Küss	−	Zeljony
	−	Zebrowski		
Swallowing	+	Brunacci		
From mucosa of stomach	+	Ordenstein	+	Jänicke
			−	Braun; Buff
Corneal	−	Oehl	+	Aschenbrandt
			−	Buff
Cutaneous, thermal			+	Parfenow
Sexual			+	Colin; Bechterew
Inhibitory stimuli:				
Visceral			+	Pawlow
			−	Buff
Protopathic			+	Fubini
Psychic	+	Brunacci and DeSanctis		

tions that have been made indicate a specific reaction for each taste substance which, with the introduction of accurate methods of giving the stimuli, will probably prove to be very accurately adjusted.

The existence of reflexes from other parts of the body than the mouth, while not absolutely proved for any particular case, is yet sufficiently supported by the existing evidence to promise valuable returns to future investigation.

Conditioned Reflexes in Man

Three attempts to investigate the conditioned salivary reflexes in man have been made. Zebrowski found that his subjects showed no increase in secretion at the thought or even at the smell and sight of food. Brunacci also obtained no evidence for conditioned reflexes to food in man. This, if I interpret him correctly, he ascribes to the fact that man analyzes the experimental situation and can not be deceived

into expecting food, as the dog is under similar conditions. Stated in objective terms this probably means nothing more than that the stimuli to conditioned secretion in man are complex situations which were not duplicated under laboratory conditions.[1] Gley and Mendelssohn ('15) were able to make a few tests in a patient with a fistula of Stenson's duct. They obtained only a slight secretion at the sight of food and no significant secretion at the thought of it. They then attempted to establish a conditioned secretion to sound and light. A series of notes was sounded on a flute, or a bright light was flashed in the eyes of the subject, and at the same time a small piece of chocolate was placed in his mouth. About 40 such stimulations were given with each (light and sound), but no conditioned reflex to either was established. The authors point out that their experiments were not continued long enough to justify the conclusion that conditioned salivary reflexes can not be established in man[2] but do indicate that such reflexes are difficult to form. Their failure is ascribed in part to the stimuli chosen but what they consider the chief difficulty is stated thus: "It seems difficult to us to conceive of the production of 'conditioned reflexes' without the intervention of psychic elements, phenomena of memory and association, images, perhaps even simple judgments, etc.; it is probable that in so complex a reaction the individuality of the subject, especially in the human species, may play a great rôle."

Discussion

The interest of psychologists has been directed to the salivary reflex chiefly as an index to the sensory physiology of animals. For this it is probably less valuable than the conditioned motor reflex since its use is attended by many technical difficulties (Watson, '16). For an analysis of the mechanism of learning, on the other hand, the study of the

[1] I have had no difficulty in obtaining a reflex secretion in human subjects at the sight of food when the subject was hungry enough to be interested and was under conditions where he had previously obtained food.

[2] In experiments with dogs 70 or more stimulations may be required before the conditioned secretion appears.

conditioned salivary reflex is of the greatest importance.[1] The method of producing it is quite simple, consisting essentially of the simultaneous application of some indifferent stimulus, such as the sound of a bell, with one which excites a direct salivary reflex, such as a gustatory stimulus. The appearance of salivary secretion following the application of the originally indifferent stimulus forms an almost ideal example of an associational as contrasted with a trial-and-error method of learning.

It presents three advantages over other methods of studying the mechanism of learning. In the first place the conditions may be so arranged that only two stimuli and, presumably, one reaction are involved. In studies of the effects of the intensity of the stimuli and of the distribution of practice upon the rate of learning this simplification of the problem is of considerable importance, but its greatest usefulness should be for studies of the temporal relations between the primary and the associated stimulus necessary for the formation of the association.

The fact that a single reaction is involved is also an advantage. The associated reaction either does, or does not, occur and there is no question of the elimination of errors or simplification of reaction. This is true also of verbal association, but in the latter case there already exists an elaborate system of word habits whose influence on experimental studies has not yet been determined. (Even nonsense-syllables occasionally touch an emotional complex.)

Finally the relative independence of the salivary reflex from the complex conditions of reinforcement and inhibition which affect the activity of striped muscles bears directly upon those theories which ascribe an important rôle to consciousness in learning. Neither 'voluntary' nor 'purposeful' secretion is possible and, normally, the subject is unable to tell whether or not he has given a salivary reflex in response to a given stimulus, yet the course of the formation of a conditioned reflex is typically that of habit-formation.

[1] Conditioned motor reflexes may be of exactly the same character but their relation to voluntary activities is not yet established.

Studies of the formation of conditioned reflexes in dogs have already revealed a number of other phenomena of almost equal significance. In particular, the lack of differentiation of the conditioned reflex when it is first established, its total disappearance with continued excitation without reinforcement by the unconditioned stimulus, and the possibility of using a thoroughly established conditioned reflex as a foundation for the formation of others are characteristics of association which have received little attention except in these studies.[1] A fairly complete account of the formation of conditioned reflexes is given by von Bechterew in his 'Objective Psychology.'

Modification of salivary secretion during emotional disturbance is a matter of common experience (Cannon, '15, cites the Chinese rice test as an illustration of the inhibition of salivary secretion by fear) but no controlled experiments have been made. A point in this connection seems to be of considerable importance for a reflex theory of the emotions. The majority of subjects are unable to tell whether or not a given stimulus has excited salivary secretion in them[2] which indicates that there are no afferent impulses from the gland of sufficient intensity to excite activity of the speech mechanism. Wm. James speaks of the secretion of tears as an involuntary process, the sensations from which may contribute to the emotional complex of sorrow. If the afferent connections of the lachrymal and other glands are like that of the salivary, any direct contribution of glandular activity to emotional complexes must be sought by objective means, since the afferent stimuli will be below the threshold of the language mechanism.

It has never been possible to obtain an accurate measure of the intensity of reaction in studies of the movements of man and of the intact animal, owing to the spread of nerve-

[1] The two latter phenomena furnish, I believe, the clue to the nature and rôle of interest and motive in complex human learning. A loss of interest clearly corresponds to the fatigue of the conditioned reflex, which seems to be the more fundamental of the two.

[2] Two exceptions to this rule complain of severe pain in the gland after an acid stimulus. Both have a history of infection which probably resulted in a constriction of some of the smaller ducts.

impulses with intense stimuli and the resulting diversity of activity. Hence, studies of the relation of the intensity of the stimulus to the organism have been restricted almost wholly to sensory physiology in the elaboration of the laws of psycho-physics and the possibility of interpreting these laws in terms of motor response has received very little attention. The ease with which the quantity of secretion of the salivary glands may be measured, the consistency of their reactions, and their relative freedom from inhibition make them especially promising for studies of this sort.

What additional facts will be revealed in the study of conditional reflexes in man can not be predicted. Even if nothing more comes of it than a verification for man of the observations already made for other animals, the interest of psychology in the subject will have been justified.

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INTUITION

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If one may trust for scientific purposes one's general experience at first hand, and the common and wide experience of numerous life-observers who write fiction, at second hand, there is now no acceptable reason for denying that this popular concept, intuition, is a live one and a real, worthy therefore of at least brief scientific discussion. Some bolder psychologists would go further, and maintain that obviously intuition is more characteristically feminine than masculine. In the light of certain modern behavioristic trends in psychology and of our new more exact knowledge of the 'subconscious,' intuition takes on a new and an important interest.

It has been pointed out that there are in the present mind of philosophy at least three more or less distinct concepts labelled 'intuition.' First (now for the most part 'of only historic interest') is the intuition of unlearned primary truth, the familiar 'immediate knowledge of first and fundamental truths.' I need only point out as we pass that this 'immediacy' of knowledge of Reid was due solely to the blank darkness of subsensory associations, especially inferences, which only now, with the mind as it really is coming slowly into its own, we are beginning to explain and to understand. As a pretender at least to 'common sense,' we may be very sure that Thomas Reid, keen analyst of his day's advances, would be today eager to accept the modern attitude toward these hidden reasoning powers of mind that have 'attained.'

A second concept of intuition is that pragmatic notion used much in common thought and speech which we here shall try, superficially at least, to analyze and then to orient: insight into foresight.

A third use of the term is the partly metaphysical usage of

Bergson—'instinct become disinterested,' discursive thought by which, although intending it at first only for its explanation, we shall finally *control* our behavior. Here again we see the hint of things that are to be better understood forthwith, that keen appreciation of the essential identity of soul and behavior; of perception, recognition, intuition, and bodily movement of some mode; which indicates as well at least as anything else the prophetic wisdom of the James-Bergson attitude:—heaven not only 'the vision of fulfilled desire' but of the bodily processes of reaching it. So far as space and time are concerned, of course Bergson's 'intuition' goes further and suggests logical subtleties in imagination at least quite too far away from the mortal behavior of the average student to come within our present scope, interesting metaphysically as it is. But so far as Bergson's notion of perception is concerned, basis of reasoning, yet somatic to its core, intuition as known, for example, to the average 'lady reporter' gets not a little clarification. Recognition and even perception means more than psychomotor adaptation,—and that more is a germ of our intuition; appreciation no longer conscious of its afferent neurility at all, 'instinct become disinterested' indeed.

Nor is a fourth use of the term intuition, seldom heard and more or less inaccurate, any further away from the basal sense-root of *insight*, namely as insight in regard to the future, especially an evil future: presentiment, foreboding. Its occasional use in this special sense suggests the generality of the term's employment among the people, but otherwise for us has no interest now.

Thus we may note that intuition means in fact, as it does in etymology, practically the same as *insight*, or at least *is* insight—insight, namely, into situations ejective, objective, or sometimes subjective, and of course into any combination of the three. I am content to rest with the simple figurative concept *insight* as a working definition of intuition until analysis of the elemental processes involved shall have extended the connotation of the process and so made our 'insight' more satisfying as a definition. Epigrammatically,

one might say it was insight into foresight. One may say then that *intuition is immediate knowledge of or insight into ejective, objective, and subjective processes or situations*, this insight involving at least the four following kinds of psychophysical event.

In the briefest possible terms, too concise to be thoroughly scientific, it seems that intuition perhaps has as its inherent character a fourfold nature: I, a delicate and sometimes ill-realized *affect* anent the intuited situation. II, a more or less accurate *process of comparison and inference* usually not at all consciously appreciated. III, *comprehension* of the situation, often with much acuteness and with far-reaching wisdom. And, IV, an effective *instinct to trust* the impression thus presented in the mind, the instinct in the adult being already long habitual.

In such a formulation we may distinguish a number of named mental processes obvious or closely and certainly implied and implicated beneath a figurative surface: *A*, keen perception, perhaps by all the sesnses simultaneously, implying sensitive receptors, and afferent centers and adequate sense-training inherited or personally acquired. *B*, delicate emotional mechanism with little starting-inertia and a minimum of muscular or glandular gross action, the 'soma' of the affect involved being largely neural,—as is the case with all long-inhibited feelings. *C*, awareness of the emotional aroma, however subtle, and *D*, appreciation, more or less consciously, that it has significance. *E*, an attempt to understand this significance; which promptly leads to *F*, a process of comparison, *G*, a judgment coming from the comparison (based on the nature of likeness and unlikeness),[1] and *H*, a more or less unconscious mysterious human mental process that we term inference. *I*, an integration of the affect and the reasoning process into, *J*, a conscious tendency to the understanding of the factors of the situation, however

[1] An experimental study of likeness and unlikeness was reported by the writer in the *Journal of Philosophy, Psychology, and Scientific Methods*, 1910, 7, 57–64. Its evidence speaks for a dynamic correlate of judgments of this kind and one more or less appreciable.

novel or complex so long as rational (including caprice) and not materialistically fortuitous; here life-experience comes in to aid the intuitive process greatly and we find the penetrating 'woman of the world' of fiction. *K*, the product of this complex fabrication (in the etymological sense!), however new or unfamiliar, is unconsciously believed in as wisdom from the soul's deeps, an instinct which involves, *L*, a feeling of self-confidence. This leads in its turn to *M*, a conscious and most gratifying *product*, and the woman knows and understands something which is of *use* to her, and often to others, in the conduct of a difficult Life, something which may protect her or hers, something oftentimes which makes her naked momentary intuition of more real worth than the male's most labored and extended mental toil of thought.

Such an 'analysis' as this of a mental action into thirteen elementary parts is of course purely artificial and withal in part arbitrary, and therefore must be taken (if indeed taken at all), as only a further amount of description of the total action as possibly it is. But the process of the insight as a whole cannot, I believe, escape analysis into at least the four processes numbered above. Let us examine them.

I. *The emotion* (affect) concerned is liable to be any one of many to which the human dynamism is liable; in 'The Influence of Joy' I have listed about eighty feelings which in theory might thus serve to direct and to energize the behavior of the intuiter toward the related situation. Sturt in his 'Principles of Understanding' (1915) emphasizes sympathy as the emotion germane to this relationship: "One common cause of slowness of judgment is defect of sympathy. A man often fails to understand other people because his interests are different from theirs. . . . The quick-wittedness of women in dealing with a personal situation is mainly due to their power of ready sympathy." But there is a danger that the natural chivalry of most writers will designate as womanly sympathy what scientifically would more properly be denoted affective interest in general. On this supposition jealousy, curiosity, fear, anger, hate, even in short any dynamic, that is affective, reaction is

adequate to attract, direct, and hold the intuiter's energy toward and into the about-to-be-intuited situation. In a broad sense of appreciation (rather than that narrow one of kindly sympathy) we may well accept sympathy as the dynamic and kinetic starter of our quadruplex intuitive process. Beneath it clearly enough is human interest in the concatenation which living forever implies, by whatever precise affective name we handle it. It alone furnishes to intuition the *vis a tergo*, the impulse, which realizes it. But the dynamism of hate is at least equal to that of love.

II. *The process of comparison and then of inference*, substantially the essence of reasoning, has for intuition no special interest other than the circumstance that in intuition the process is at once quick, accurate, and wholly subconscious; yet that seems to be adequate reason why it should interest descriptive and explanatory psychology. Reasoning is a basal mode of mental activity existing in its essence everywhere, and always typical of the truest relationship of the ego and its effective environment, at least in those stages of this relationship when instinct in truth has become 'disinterested,' as Bergson says, 'and capable of reflection.' It is "formal logic" that has made reasoning unpopular; but thought must be libelled no longer as somewhat too recondite for scientific discussion, for it conceals in a mass of dialectical chaff grain too real, too dynamic, and always too important to longer remain lost. And logic will be hateful no longer than whatever length of time the dynamic theory shall require for the statement of the kinetic relationship of ego and environment in terms that all may understand. From the dynamic viewpoint the psychology of thought is seen to be still *alive*, and not merely Medieval and Scholastic.[1]

III. *The comprehension of the intuited situation*, our third intuitional component, cannot be dismissed so curtly, for it involves not only a suggestion of the intuitive relationship, but a brief connotation of the concept 'situation' which we have so often already employed.

[1] See, for example, the writer's "How to Learn Easily" (Little, Brown & Co., 1916), especially the fifth chapter. To set forth somewhat more in detail the kinesthetic aspects of ideation is a task already begun.

The term comprehension is a fit one here, for it should mean for our present use just what etymologically it ought to mean — a taking-together, an understanding of some integration in its parts as well as in its wholeness. The integrated parts in this intuitional process are of the same derivation and of the same general nature as the factors of understanding elsewhere. Sense-impressions, often marvellously subtle, from without, percepts, stores of concepts within, memory 'fringes' of experience first- or second-hand, innate tendencies to integrate or to analyze, kinesthetic 'vestigia' serving as cues to imitative interpretation—all that undescribed, but inherently describable, vast complex of conscious and subsensory mentality which expresses for us *the meaning of behavior*, be it in ourselves primarily or in others. The clever 'clairvoyant' of today, the Stone-Age priest of old, Hans the horse, 'Sherlock Holmes' all had this comprehension clear because these intelligible elements to them were explicit, and symbolic of meaning. For business purposes the emotion need not be obvious, indeed may be conspicuous by its absence in the hackneyed 'palmist' and the kind of comprehenders whom she typifies.

But what does one mean by the intuited '*situation*'? Inasmuch as intuition has no assignable limit of range or acuity it certainly were illogical to limit, either, the situation which the process explains. On this basis I propose to simply denote situation (here as elsewhere, wherever in short, reality is in relation with consciousness) as *any appreciable relationship whatever, ejective, objective, or subjective, so long as not irrational*, and including, therefore, pure caprice. By irrational we can mean only fortuitous in the metaphysical sense, but to attempt justification here is quite inexpedient, since the mutual bounds of chance and rationality, of chaos and cosmos, no man can pretend to set, and less and less may he pretend to do so with the advancement of learning and of insight into nature.

As any appreciable relationship whatever, then, in the rational world, 'situation' is delightfully simple to denote, but obviously only because it is inexpressibly and unimagin-

ably too various and too complex oftentimes for words to justify at all. And this viewpoint really *needs* no justification as long as kinesthetic vestigia supplemented by other immediate sensorial data can serve as cues to the beginning of the intuitional performance; for thus long even the human mind will consciously and subconsciously realize its environment more or less fully.

But of course a considerable proportion of actual intuitions or intuitive actions in our civilized human experience deal with a narrower range of situations, namely with those primarily ejective: intuition most often 'sizes up' some person's relationship to his environment either at a particular time and circumstance or else in general, as a habit. In the former case the intuiter would learn, by intuiting, what the person would do next, and the series of his behavior; while in the latter case the inquiry relates to that person's character. And here one sees part of the biologic answer to the question why women and girls are the expert intuiters among humans; obviously to protect them automatically from the strenuous and often unimaginative and therefore 'selfish' male. Indeed, it were not easy to overevaluate intuition in this respect to young women, sometimes and in some circumstances so irresistibly seductive and yet so helpless and so liable to irreparable harm. A common, the most common, intuitional 'situation' is, then, human nature and its behavior, and, most specifically common of all, men's characters in biologic relation to the intuiter, when women's characters are a necessary part of this complex but theoretically simple sexual, biologic situation.

IV. *An effective instinct to trust* one's intuitions was our fourth factor. It is plain that without this the rest were futile, and it is just as plain that endless generations of experience (say 30,000 of them?) have long since made this trust a coherent part of the mental process we are attempting to elucidate. This mind-confidence, so conspicuous in strongly feminine women and girls, which makes them trust their "feelings," their intuitions, their inspirations, impressions, over-soul, or whatever else it be termed at times, is

perhaps best made obvious to males by observing how frequently it is essentially lacking in themselves. Men trained by intense need and vocational practice to making quick judgments, be they trivial or be they important, do acquire a degree of true intuition. Frequently, too, male bluster pretends a true quick choice when in reality the pronouncement comes 'by chance,' at random—or else by true intuition. But, on the other hand, many men—those, *e. g.*, of 'judicial temperament'; those intelligent enough to know the supreme value of subconscious consideration; and those upon whom rests as a burden the responsibilities of life and death and happiness and woe—realize perfectly well that they *must not* trust their impulsive judgments too implicitly, for too frequently are they irretrievably wrong. And, if we cut out of our observational experience all cases of pure male obstinacy and bluff, with might to make it seem right, how much stronger is the proposition that men usually do not have this 'effective instinct to trust' their impressions which is so vital a factor of intuition.

With these four indispensable components of intuition in our minds 'on approval,' we would like next to examine into the physiologic aspects of this type of behavior. But we will not do so, at least not at this time. Here it is enough to point out that the behavior of the intuiter is that of enthusiastic intellectual emotion, of thought, of inward attention vivified by some feeling wholly acceptable and pleasing to the subject. The characteristic outpouring of adrenin, the characteristic heart-rate, blood-pressure, breath-rate of intuition we do not as yet know.

By way of a moment's respite, of restful change of focus, let us consider here that in general it is one of the "silent mysteries" of mind, why psychology does not more rapidly study the often practically important tertiary sexual characteristics. It is possible that some would insist that there are none,—none, at least, worth their valuable time and effort; that this bottomless crevasse of sex which so completely divides the entire living world in all other respects

into two opposed yet complementary halves, does not extend into the peaceful animistic realm of mind. For my part, however, I do not believe it, for I see a forbiddingly large fraction removed from the human nature which we psychologists crave to understand, were sexual mental differences entirely smoothed out. At any rate, the present remarks depend upon the presupposition that feminine intuition is a fact in need of study, together with its implications, and one of the most obvious and most important of the tertiary sexual characters—another of the complementary aspects of the sexes so much exploited by the phrenologists yet vitally important because so true. Havelock Ellis has set the pace—who follows in his train?

If the reader glance back over our intuitional analysis, it is conspicuous that a constant element of the 'situation' always implicated is the interrelated *character* of something or other, good or bad, trivial or momentous, living or nonliving. Character, as Fichte and Royce and Emerson and wisdom in general tell us, is inherently purpose:

> ". . . , perseverance, dear my lord,
> Keeps honour bright: to have done is to hang
> Quite out of fashion like a rusty mail
> In monumental mockery."
> ('Troilus and Cressida,' III, III.)

Simple purpose, then, perseverance, and the rest, stand for much of the situation with which we deal; and at the base of intuition stands solid and strong the appreciation of motivity, an interpretation of human purpose, an habitual and therefore automatic tendency to put one's self in another's place, to make his problem seem as much one's very own as if it were so. Oftentimes, of course, the situation involves the motivation of numerous persons, not alone of one, or the comprehension even of a whole 'social consciousness' itself. The quintessence of the process seems to be most usually an appreciation of *motivity*, of primal cause, but more typically when in others than when in one's self.

With Professor Warren's acute analysis of purpose so

recently before us (see the *Journal of Philosophy, Psychology, and Scientific Methods*, Jan. and Feb., 1916) it would be perhaps superfluous to further seek the introspectible factors of motivity, for the two problems purpose and motivity certainly widely overlap. 'Forethought (anticipation); assent; potency-feeling; the self-notion; and the sense of fitness' are the five parts discovered by this observer in purposiveness. The assent, potency-feeling, and the sense of fitness are declared kinesthetic, and yet incidental; while forethought, 'the most characteristic factor, is an idea or image which carries with it a reference to the future . . . a real biological phenomenon' when kinetic; and the sense of fitness is a 'judgment that the experience corresponds' to the anticipation—'a factor very characteristic of purposive experiences.' So far we may follow, but when it is stated that purposive activity (and this alone concerns us in intuition) 'is characterized by only two of the factors noticed in the conscious experience,' namely preparation and adaptation, we have to express dissent, and wonderment as to how the adaptation is brought about else than by 'assent,' 'the potency-feeling,' and 'the self-notion,' each clearly kinesthetic. Without these, the 'entelechy,' so properly condemned by Warren, seems actually inevitable in the adaptive process.

Intuition, then, seems to the present writer to involve *the appreciation by the intuiter of the potential purposive activity* of the intuitee, the very essence of which activity is given to the former in ill-appreciated adaptive, that is kinesthetic, terms.

The intuiter feels in short the action, the behavior which the other's attitude toward his environment properly demands on his part. Then without realizing it clearly, she takes it for granted that the natural activity will eventuate—usually without error, as her long ancestral experience has found and, down the ages, made innate. Women, no more than men, have clairvoyant powers, the ability to read motives and thoughts, etc., on any basis whatever other than that of their own personal or inherited experience. And this experience, so far as related to activity, certainly seems adaptive,

that is, in its psychologic aspect predominantly motor and kinesthetic. The very essence of conscious adaptation psychologically is plainly kinesthesia, just as the essence of intuition seems to be adaptation in its potential phases in someone else.

This kinesthetic criterion on which a person may intuitively grasp an 'ejective' situation would seem in part to account for the emotional tone in the recognition, and still more surely for the understanding of its personal nature,—two of the four suggested factors of intuition. *Interpretation is wholly blind to us save on the kinesthetic basis*, as the writer has tried to show in some notes already printed on kinesthesia. Thus intuition stands not only for exteroceptive perception, but, in a still more intensive way, for proprioceptive sensitivity and appreciation. Kinesthesia is the very warp of all perception with the varied woof or 'filler' coming from the other senses and their 'centers.' Thus the modern dynamic explanation of behavior cannot possibly be ignored in any discussion of intuition, any more than the thoughtful physicist who is read up to date can slight the kinesthetic origin of our racial mind's concept of energy, force, and work. Philip,[1] by correlating with Newton's laws of motion, has already shown how far-reaching is this relationship into our basis for every 'situation' not purely metaphysical—if any such, indeed, there be as one *purely* metaphysical.

Again, then, it must be noted that the intuitive capability, like all others like and unlike, depends absolutely for descriptive psychology on the dynamic relations symbolized and indexed by *kinesthesia*, *the dynamic mental warp of our behavior-fabric.* It is interpretation in terms of the known and in a manner much as Royce improved on Tarde in regard to imitation. It alone makes intelligible whence an intuitess derives her awareness of expending energy, of stress and strain and shear, of causality, of a whole causal series, out yonder in that situation which she so quickly, quietly, but keenly evaluates. She is able to put herself in the other's place and to feel what that other feels and to know what that

[1] A. Philip, "The Dynamic Foundations of Knowledge," London, 1913.

other unerringly (barring caprice) will do, *only* because personal or inherited experience of such dynamic situations has given her an insight, as certain to her as daylight, into the kinetic series of events that must come forth. On any other known basis than this, the dynamic or kinetic basis empiricized as kinesthesia, dynamic index to our souls of our effective environment, the complex intuitive process has no meaning simply because it has no substance other than one *too esoteric for science to discuss.*

William McDougall in his "Body and Mind" is certainly rather ingenious in his arguments that meanings are independent of the action of the neuro-musculo-glandular co-ordinations. Yet it seems to the present writer ample refutation of such an unscientific presumption to remind all and sundry that *the only conceivable means by which these meanings as psychograms ever could have become explicit in the mind is through the perception-process, elaborated by central association.* Perception and association partly are bodily actions. Moreover, it is undeniable that these bodily neural actions persist, constitute the dynamic framework, so to say, of the active mind, and must be repeated, overrun, whenever a meaning, however subtle, suffuses the personality. These things are at the very heart of our understanding of behavior; as a neo-animist the writer, for one, does not wish to do else than to explain and to accept them.

Objective perception, perception of objects, with their recognition, meaning, and other relations get their actual start in the comprehension of including situations. Objects have no forced connection with our minds at all save as they represent for these minds and for behavior something with meaning, real meaning, use—save, in short, as they are interpretable in terms of our own activity. Sturt has made this really important matter as explicit as possible in his recent treatise on the Understanding already referred to: "It is important to notice," he says, "that in the order of mental development [phyletic and ontogenic], purpose and cognition of situation come before the recognition of distinct objects, *i. e.*, before the apprehension of persons and things.

To a superficial observer the order is reversed. . . . Animals and even men living under purely natural conditions, notice only those objects which they can use." Then Sturt illustrates by suggesting the contrasts in the behavior-reaction of a caterpillar, of a kitten, and of an author toward a fountain pen. "We may conclude, then, that creatures of the lowest intelligence cannot be said to recognize objects but rather to perceive objective systems or situations corresponding to the purposes whereby they seek satisfaction of their desires" (p. 203).

While this probably is as true in general as it is important and keen when applied to the phyletic animal range, it apparently is not observable in the actual human mind as a dominant principle of action. In fact, if I can adequately judge from the protocols so far received of an experimental study (in progress) into the psychology of meaning, there is a distinct, but perhaps not large, correlation between *efficient intelligence*, in a broad practical sense at least, and the tendency to apperceive *meaning as active behavior*, as action rather than as objects proper unrelated to activity. This empirical attitude toward meaning is most obvious in a comparison of morons with normals, but, unless I misread my experimental results so far, it is also discernible in comparing thoughtless ('shallow') normals with those who by habit think into the real relations and into the dynamic essentials of things. Meaning is for some non-existent in a given character or situation; for others its essence is some object; for others its import is its action on behavior; for still others its meaning is frankly only its active relationship to an active environment. *Humans* of 'low intelligence' apparently may *not* be said to 'perceive objective systems or situations corresponding to the purposes whereby they seek satisfaction of their desires' or anything else, for if they perceive anything at all that is denotable, it appears to be uncompromising objects, stark and passive, relatively unrelated to themselves or to the remainder of the always energetic environment. Lack of *intuition* obviously comes in here—lack of insight into the purpose and the realest mean-

ing of things. The implication of these above considerations for our notion of intuition meets the reader's understanding face to face.

From several angles, then, we may presume it demonstrable that the entire intuitional affair, save its product, is one of the highest possible *intelligence* and at the same time characteristically subconscious. In fact, no better example of this common association in mental activity of keen intelligence and absence of conscious process, is at hand than this. If we think, with Münsterberg for example, of the soul as 'a system of purposes which remains identical with itself in developing its potential acts as real experiences' we have in intuition, as almost nowhere else, a typical example of soul-activity. On such animistic basis, shall we reject the existence of psychological subconsciousness? Here if anywhere is that meaning which lends causal connection to unconscious intelligent associations.

It certainly is something outside the range of physiology and of neurology—and therefore within the range of mind—that one person can by processes of reason and of feeling, often wholly unconscious, comprehend a situation characteristically spiritual in another person's mind,—a motivation-reaction of that second person to his environment. Why do a few still let a matter of arbitrary definition refuse to events as clearly spiritual as this the adjective *mental?*

Psychology surely has and need have no fear of losing its scientific identity, of being gulped down and digested by physiology or by neurology, when so large a horde of problems plainly mental and evidently causal like this one press upon it in a crowd for immediate solution. Physiology here, in such a case as intuition, has not much satisfaction to offer that will convince the unprejudiced. Physiology certainly offers for study, and ultimate understanding perhaps, the *mechanism* of so recondite a common experience, but it does not offer—nor can it ever, it seems to me—that unexplained remainder by which woman's sensitive keenness and woman's sympathy leap the restrictions of even the human nervous

system, of the musculature, of adrenin, of *energy* vitalizing the stupendous cortex of the human brain and giving it these unique capacities. And what of it, if part of this spiritual process is in the dark? Is it material, neural, because of that? If not then material, is it not mental? Nor is the true believer in the propriety of terming subconscious processes distinctly mental, going to consent much longer to be ruled out of court as a proper student of psychology on the trite old argument that psychical succession offers 'no causality' to the uniformity-seeking mind of the scientist. The new soul is certainly coming back and the *new* soul means something! An 'asymptotic regress toward a pure subject of knowledge' no longer, the 'inner' purposiveness and the 'outer' activity are joining hands joyfully in full sight of the unlearned man, who bids them glad welcome in the positive hope at last that these two made one will help him to really understand himself—if not his God.

Intuition, then, and the comprehension of a total situation of whatsoever kind, involving motive which intuition implies, is in a way and in a degree a real criterion of real intelligence in its most significant values. From this deep way of looking at the matter, the feminine mind is more evolved, more *intelligent*, in short, than is the mind of the male. Obtuseness stands for abnormality or for relative lowness of human grade; intuition for a high degree of that which mind is especially meant to serve—the safeguarding and the furtherance of the individual. From such considerations it would appear that intuition deserves far more study and consideration than thus far it has received.

Its abnormal conditions, especially its derangement or its lack, have not received at the hands of the test-systems the attention which they inherently deserve. In my intimate blood-pressure work with mental defectives I have had an excellent opportunity to compare 'psyche' and 'soma'; the more subtle phases and aspects of mind proper with the familiar conditions of muscle, circulation, respiration, motor control, automatism, etc., which 'underlie' us as humans in common with the brutes. Passivity merging into obtuse-

ness is conspicuous. The contrast between the fine active physique of a middle-aged Negro farm-hand and his 6.6 yr. point-scale intelligence, as shown not only in his hemobarogram but in his social (extero-active) behavior, is an instructive thing for several interests. There is obvious a lack of the subconscious associative niceties; the term astuteness does not pertain; that natural eagerness to finish and do something else is commonly lacking; extraneous and future comprehension, planning, purpose, are not there. The childish men and women exhibit no intuition and no curiosity as to the reason why I work so intimately with them for an hour or less, but they show plainly enough sometimes all the intuition that a six-year normal girl exhibits that the soft silk and rubber cuff of the pressure-gauge may hurt them. In one instance a woman 42 years old, 1.6 years Yerkes scale, burst into tears as she felt the unfamiliar constriction-sensation in her arm. Mental defectives, especially female, have intuitions surely enough, but they are of that simplicity of 'situation' that they merge into the most basal of the emotions—intuitions in a sense, carnalized and made hereditary by thousands of generations of continual activity, reduced so that the 'situation' has resolved itself into the relatively simple 'object' of the emotion, usually its occasion.

We come then to another sanction for discussing intuition: it is a criterion of the practical reason, of human sanity. We have failed above to be intelligible if it is not now obvious that the general practical intelligence of any animal, brute or human, has a valid index or criterion in that animal's 'intuition'; and so too for the lower ranges of the human intellect and the intelligence deranged. Here, in fine, is a definite criterion of mental normality. The writer has already briefly discussed this in an earlier article from which two paragraphs may be quoted by way of reiteration:

In attempting to define the difference between normal and abnormal mind we have come as a final criterion to something at least theoretically more satisfactory than either of

the preceding considerations. It emphasizes anew the both vulgar and classic opposition between the common and the egotistic good, between altruistic benevolence and selfishness. As an individual and with only individual responsibilities and duties the deranged man's conduct may be as satisfactory and complete as that of one classed empirically as sane. His nutrition may be as good, his form as sleek and comely, his strength normal, and his body to all appearances, perhaps even microscopic, wholly sound. Even the mental aspect of his organism may possess all the requisites of proper function—good sleep, clear ideas, memory unimpaired, imagination very likely better than the average, sensations and feelings normal, and, be it emphasized, conscience wholly clear. With all of these and what they imply, with soul and body in evidently proper order, a man or woman may yet be the most dangerous of lunatics, the maddest of the mad. But turning to the vast *social consciousness* of which this willing subject is part, inquiring in what degree of harmony this person's life-purposes stand therewith, and the discrepancy at once is seen, his lack, the nature of his inward dissonance, the reason for his life none too much confined. Humanity's consciousness, too, has purposes and plans, and they proceed inevitably to their grand fulfillment. It is because the purposes of his suspected person (and whether free agent or automaton matters not here) run counter to this evolutionary current, that unavoidably he is overwhelmed and forced beneath and drowned. Neither anatomical nor physiological nor psychological nor yet personal, in a sense, is the deranged subject's defect, but it is *sociological* and against the evolving purpose of the race. Any given case may, of course, be defective in each of these respects, as most are in some of them, but the defect essential to abnormality certainly finds its place relating far outside the individual in the complex intention of the race, be that intention moral or unmoral, racial or cosmologic, ill or good. This is the one unrelative standard by which all vital subjects may be judged. To be insane is to be out of tune, not with the laws of psychology or of physiology, nor yet of the state,

but with a broader and more essential tendency—the purpose of the world, it may be of the universe.

To this conclusion not metaphysics alone, but also empirical science points. *Reason* is the aspect of mind which men in general have deemed the highest and most dignified power of soul, be it called by whatever particular name. It is that endowment by which certain species, or certainly one species, of the animal kingdom understands the most real nature of things and their relations—the *meaning* of the ceaseless life of change in which all our experience is passed. Reason is the just comprehension of cause and effect, or common sense. Now only a part of the accepted varieties of insanity imply disturbance of this, the crowning power of mind. Mania, for example, is only an unusual hurrying of the psycho-physical action of the higher animals, involving as essential no disturbance other than one of a temporal sort. Melancholia is, on the other hand, the reverse of this, a slowing of the life, a too long continuance of painful thoughts. Dementia paralytica is more of a bodily disease than the two just mentioned, and epilepsy, dementia, idiocy, even more. It is in paranoia that we see a loss of reason in the technical sense of the word, for it is a disease characterized by a confusion of the relations of cause and effect quite as much as by the systematization of delusions based thereon. Here is a state of mind wherein the meanings and purposes of things are deranged, a general disturbance obtaining in the egotism of the subject leading him into greatly wrong relations with himself, with things, and with society at large. This is the typical instance of mental abnormality where is seen at once perverted psychic action and a basal disagreement with the racial plan.[1]

Of this out-of-tuneness, of this fundamental disharmony or discord with the social values, there is not a more accurate nor a conciser concept than intuition. It expresses more in a small space, this 'word-handle' of a general idea, than any other that may be found, perhaps, explanatory of *the practical effective unreason of man*, and that too without betraying the

[1] Psychol. Rev., 1898, 5, 506–510.

necessary individualism of a valid philosophy; and this thing some other criteria of mental abnormality cannot do. Were we somehow less dependent than we are on our spiritual environment, this intuitional criterion would be less representative. But as it is, as we are, intuition expresses very much, for it suggests directly that appreciation of the basal life-relationships, *causal*, rational, social as well as psychologically personal, on which alone our whole important concept of abnormality has any modicum of meaning.

It can be practically applied as a criterion, as a test, far more elaborate than Healy's pictorial completion device; which however is clearly along this line. Class study by means of a questionary given to normal students and collegians would soon develop data which would test a student's native intelligence somewhat better at least than the absurd agonies in algebra over which so many discourageable boys and girls are just now worrying. But tests are outside our present situation.

Incidentally it should rejoice every man and boy that the biologic source of our very being, womanhood, undoubtedly is the richer of the two sexes in this intuition, this useful measure of our common human always yet divine intelligence. Is not this richness a criterion in a way, of woman's superior intelligence?—not perhaps to politically govern in this world which is still one of *might* and maddening terrors of force and blood, but in all of those more permanent and less savage aspects of mentality which will watch our earth 'grow cold.' Yes, even at a risk of supplying ammunition to the suffragettes, the writer for one must maintain that the intelligence of the human female, tested by some of the most basal and certain tests in any 'system,' is the psychical superior of men. If the general scientific recognition of this interesting fact come late, it is largely because the criteria of superiority have been below the proper human ideal. When at last *skill* shall have replaced *strength*, the general recognition will have come, and a new chapter be written in psychology.

THE INTELLIGENCE EXAMINATION AND EVALUATION

A Study of the Child's Mind

(Second Report)

PART II.

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New York

IV. The Tests for Comprehension

Under Comprehension, we first test (*a*) the *comprehension of numbers.* Pennies (and stamps or stones) may be used. Binet found that at four, half the children tested count four pennies, while at five only the retarded fail. At six one third of the children count 13 pennies correctly, at seven all of them.[1] In a later test Binet used three single and three double sous (for which Goddard substituted one and two cent stamps). In this test many succeeded at seven, all at eight. Binet has the child touch the coins consecutively. Ziehen, on the other hand, who in testing numbers suggests 4 to 7 or more small stones, does not permit the child to touch these, nor aid in the task through moving of fingers or nodding of head. He asks, "How many stones are there?" Only if this cannot be done does he allow the child to touch one stone after the other. (The latter is the optic-motor counting, the former the purely optic, and the former entails greater abstract number comprehension and is more difficult. The latter may often be accomplished even by lightly imbecilic children—as also the counting of sounds [acoustic impressions]—up to the number of 6 or even 10.) The severe imbecile and idiot have mostly no conception of num-

[1] Bobertag, *l. c.*, p. 65, holds this test too easy, and believes that almost only the feeble-minded fail in it. For him it is scarcely a 'psychological test' but rather a pedagogical one. The same is true, he thinks, of the test with three single and double sous.

bers whatsoever, or only to 2 or 3.[1] One may likewise place 20 stones before the child, and say, "Give me 3 stones, or 5," etc.[2]

In making change one plays at 'make believe' with the child—as Binet suggests—giving him a quarter, and buying 4 cents' worth of something or other. The correct change must then be returned to you. The child has put in front of it real change in the form of 10 pennies, 2 nickels, 2 dimes, 1 quarter and a half dollar.[3] All succeed at nine years. Children who come in touch with money will, of course, do these tests better than those who rarely handle it. Ziehen, however, points out that even severe imbeciles at times gain a very fair knowledge of coins.[4]

One may also ask, "Which is greater, 6 or 10; 100 or 50; 100 or 500?" "Which is less, 10 or 5?" etc., "Many or a few?" But in these questions we are leaving the concrete and involving relations.

Under (*b*) we ask the child—according to Ziehen—to *isolate* certain qualities or parts of a familiar composite. "What are the qualities of sugar?" (whiteness, sweetness, etc.). "Of what is a storm composed?" or "What is a street, summer, a city, a concert, war?" etc. The opposite process, that of melting together single facts into a whole, as of rain, thunder and lightning into the concept "storm," may also be tested. "What is it" one will ask, "when it rains, thunders and lightnings?" or "What do you call it?"[5] This is termed '*complexion*' (Komplexion).

Still another psychological process belongs to this group

[1] It is interesting to note that the comprehension of numbers comes late in the development of intelligence. The lower animals have scarcely any real knowledge of numbers. It is but little better with primitive man. The aboriginal Australian can rarely count his five fingers, and no Australian language contains numerals above four, all numbers beyond this being described as 'many.' See Sully's 'Outlines of Psychology,' p. 283–4.

[2] Ziehen, 'Geist. der Kind.,' 1915, p. 66.

[3] These coins have been suggested in place of the French pieces.

[4] 'Die Geisteskr. d. Kindesalt.,' 1915, p. 63.

[5] Ziehen notes that most feeble-minded children get the partial idea of a composite more easily than the reverse; *i. e.*, if you ask "What is a storm?" they may answer, "It rains, it lightnings, it thunders." But when the question is put as above, "What is it when it rains, etc.," they cannot answer (*l. c.*, p. 63).

and may be tested. When a child forms the conception of 'bird' from seeing many birds and noting the resemblance of many animals that have wings and beaks and that fly, or the conception of 'furniture' from seeing many pieces of furniture, this is termed '*generalization*.' Hence, we ask, "What is the general name you give to eagle, goose, sparrow, swan, etc.?" (Or the general term for chair, table, sofa, etc.) One may also reverse the problem, asking for 'all the birds you know,' or 'all the pieces of furniture, or trees, flowers, tools (adverbs) etc., you know'[1] (or, better still, ask 'name me *some of* . . . ,' etc.). Only the defective child would name snail or frog among the birds.

These three processes, *isolation*, *complexion* and *generalization*, Ziehen[2] holds as fundamental in the building of ideas. The three processes constantly interlace in the most intricate and manifold ways, and generate our innumerable conceptions and ideas. The child, very early, through these processes notes relationships and sizes up the unfamiliar. It is through generalization that we classify and establish order in our immense accumulation of separate and incidental facts.[3] Generalization is also abstraction.[4]

Though these three processes may be examined separately,

[1] This is termed "*specification*."

[2] Th. Ziehen, 'Leitfaden d. phys. Psychologie,' 9th edit., Chap. VIII.; or 10th ed., p. 292, et seq.; 'Intelligenzprüfung,' 1911, *l. c.*, p. 29. See also W. Preyer, 'Die Seele des Kindes,' Leipzig, 1905, p. 235.

[3] So Meumann also writes: "The fundamental process of thinking (Denktätigkeit) which no doubt commences in the child with the earliest beginnings of speech, is the classification of ideas according to grades of generality (or generalization—'Allgemeinheit'), Vol. II., p. 426. That one idea falls under another, continues Meumann, we know instinctively, and without calling up clear definitions of both. Meumann makes his tests as follows: He says to the child, "I'll name a thing and you are to give me the class (genus) to which it belongs," and he elucidates with an example. With younger children he gives examples of analogies, and then, placing a written list of words before the child, asks it to do the same.

[4] That is, generalization is dependent on abstraction; but abstraction does not involve generalization. Thus Hamilton says as to forming abstract general notions: "this is done when, comparing a number of objects, we seize on their resemblances; when we concentrate our attention on these points of similarity, thus abstracting the mind from a consideration of their difference; and when we give a name to our notion of that circumstance in which they all agree." etc. (Hamilton's 'Metaphysics,' Bowen edit., p. 465–7.)

they are probably best probed collectively in the following test (*c*) of *Differentiation.*

In this Differentiation test the point of *comprehension*, as such, is more readily focused upon, and without forfeiture to language ability,[1] chance, etc., as is the case in the matter of defining (see next test), and, as Binet notes, does not depend on instruction, but brings into play the natural good sense of the subject.[2]

Ziehen gives at least 4 to 6 sets (see our sheet), and affords the child ample time to consider. Binet, on the other hand, gives three pairs (butterfly and fly, wood and glass, paper and pasteboard),[3] two out of which must be correctly answered, and this must be done within two minutes in order to pass. At six one third of the children succeed, at seven nearly all, and *at eight*, *all.* Bobertag's statistics are less satisfactory: at six but 5 out of 30 passed, at seven, 52 per cent. of 42 children, at eight, 73 per cent. of 40 children; at nine, 93 per cent. of 33.

In a more recent publication[4] Ziehen gives the following age norms: Hand—foot, at the latest at five; cow—horse, at five to six; bird—butterfly, water—ice, at seven to eight; tree—bush, lend—give, at nine; garden—woods, at nine to ten; mistake—lie, at ten to twelve. This applies to the city child. In country children the age norms may be somewhat different.

It is probably best not to ask blankly, "What is the difference between . . . ," although the older children will understand this; one had better give an example, or explain the matter. Thus, if the first question is not understood or answered, Ziehen asks, "How do you tell, on the street, that an animal is a cow or a horse?", and if this fails, 'are they birds or flowers?' etc. Binet proceeds as follows: "You know what a butterfly is?—and you know what a fly is, too? Are they alike, a butterfly and a fly? Why not? In what way are they not alike?"—and in the same way the other

[1] The debile (or moron) of fine family may be far ahead of the normal uneducated individual in this respect. (Ziehen.)

[2] Binet terms this test the comparison of two objects from memory.

[3] Goddard also gives as alternatives 'horse and cow; stone and egg; grass and tree.' Meumann first asks (*a*) for the differences, and then (*b*) for the similarities. Among his pairs, beside the above, are box-basket, streetcar-train.

[4] 'Die Geisteskrank. des Kind.,' 1915, p. 68.

words are taken up. A real difference must be given. Yet some experience is necessary to know how to evaluate a reply. Such an answer as (to the words 'steps—ladder')—"In the ladder air is between," must, according to Ziehen, count as correct.

In commenting on Knowledge, I said that knowledge was really a 'differentiation of matter'—a *differentiating* between things. The normal process, in the young, probably, is first of noting similarities—assimilating—and then differences.[1] But directly a child begins to observe, and this is very early, both processes take place simultaneously. "Although in forming the concept 'animal,'" writes Sully,[2] "we are explicitly setting forth similarities among diverse things, we are implicitly marking off the class from other things (plants and inanimate objects) which lack these similar features . . . . Thus the process of defining a class-name includes in its most complete form an examination not only of things denoted by the name, but also of things not so denoted, in order to see what features they are wanting in. This consideration of differences becomes a prominent feature in the marking off of one idea from a kindred yet partially dissimilar idea, as metal from mineral, wise from learned, and so forth, a process that plays a large part in the definition of general names. Finally, in what is known as logical Division or Classification, where things are systematically arranged in higher and lower groups, attention is paid at once to points of similarity and to points of difference."[3]

When we come to the subject of *attention* it will be seen that the feeble-minded child's attention flickers too much and rests nowhere long enough to observe sharply, hence its power of differentiating is decidedly thwarted or nil. This is especially brought out in the matter of abstract ideas. On the other hand, the better mind is keen in observation (and

[1] Not that one is more difficult than the other—though it is usually assumed that finding similarities is easier. The difficulty depends on what things are to be compared, and what knowledge the individual has of these things. See Bobertag's comment, *l. c.*, p. 96.

[2] J. Sully, 'Outlines of Psychology,' p. 286.

[3] *Cf.* citation from Hamilton, p. 486, footnote.

accordingly acute in differentiating), and for such knowledge rapidly grows.

Test (*d*) that of *defining*, is not a very safe test, at least in Binet's form, nor by any means easily evaluated. Definitions in themselves are difficult. They involve not only language ability but language dexterity. Even for adults they prove sticklers, having a tendency to disconcert, and causing confusion. Ask an adult, for instance, as Bobertag suggests, the definition of 'a whole,' 'a condition,' or, 'what is meant by truth'! Children will frequently parry by giving an example instead, *i. e.*, "What is wisdom?"—"Wisdom is when you are overheated and don't drink ice-water."[1] Many definitions, too, with children, are school-drilled, and show up memory—scarcely always good memory—not comprehension. So Gross (*l. c.*) tells of a twelve-year-old girl giving as definition of table: "A quadrangular board (or four cornered) with four legs." This answer also shows how the child is wont to use a single instance to represent the *general* conception (for there are round, oblong and other tables, too). Thus, again, "What is a thing?"—"A thing is a table." "What is a sheet?"—"A sheet of paper."[2] There is no doubt at all that the boy giving the above example of wisdom had quite the proper conception (within a boy's limitations) and could *differentiate* between a wise and an unwise thing. But what a muddle he might have made of it if forced to stick to a definition proper! So we ourselves, when striving to make things clear to the conception of another, without too much effort, use examples instead of definitions. Hence, the parable. In fact, the logical classifying of things is accomplished in most people (according to Meumann) with instinctive certainty, even though often enough a definition of the idea cannot at all be given.

Nevertheless, in spite of these seeming difficulties, the

[1] Cited from Messer by K. Gross, 'Das Seelenleben des Kindes,' Berlin, 1908, p. 215. Other like examples given (cited from F. Wolff) are: "Tall—is if a tree is very big"; "Vain—is if you always look in the glass."

[2] Still another characteristic of children's defining is their bias toward action, and especially in relation to use or purpose. "Poison—is what one gives to mice"; "A knife—is to cut meat with."

definition has been admitted as a desirable test of the child's intelligence, inasmuch as the ability to define appears to undergo certain characteristic changes in the child's mental development. So, Meumann[1] notes as many as six changes, or methods of reply, only three of which I shall give here:

1. (The earliest) by replying with an example.
2. (*a*) By defining through terms of usage or employment;
   (*b*) Or, in the case of a person, by naming at least one of his chief activities (*i. e.*, "What is a king?"—"One who rules his people");
   (*c*) Or, in the case of abstract ideas, the statement of at least one inherent distinguishing feature (*i. e.*, "goodheartedness—is helping one who suffers").
3. Through analytical description.

Etc.

Binet, on the other hand, recognizes three methods (définition d'objets), the *first* being with gesture only, the child pointing to the thing, and remaining silent, or again, saying, "It is this," or by repetition (*i. e.*, "What is a fork?"—"It is a fork"); the *second* by defining in terms of use only (*i. e.*, "What is a chair?"—"It is to sit on"; "A horse?"—"To pull a wagon"); the *third*, in terms superior to use. He assumes the second type of defining as normal for the average child in the sixth year (at four half the children do so, at six practically all); and the third type for the average child in the ninth year (half the children in the seventh and eighth year defining thus). The words he employs for the tests are 'fork, table, chair, horse and mama.' Three out of the five must be acceptable to pass the test.

Bobertag[2] subjects this defining test to a thorough and excellent criticism. For him it is one of the most interesting in the series, though, in the form given by Binet, *wholly useless*. The fundamental idea however is very good, and may be made serviceable. Bobertag, like Binet, concludes that most 5- and 6-year-old children are able (either 'without'

[1] 'Vorlesungen,' *l. c.*, Vol. II., p. 430.

[2] *L. c.*, p. 50, et seq.

or 'with' a little necessary coercion), to define, or rather *explain*, all or several of the five given words in terms of utility. A few, he found, will answer through repetition, and a few in some nonsensical way. As the age advances these answers in terms of use decrease, but do not entirely disappear. Answers in terms of material, or through description, also occur, but are not common, and, likewise, lessen with the advance in age. On the other hand, answers in terms of class names (genera) which before the age of eight almost never occur spontaneously, now increase and finally obtain (save for a few utility answers) almost entirely. There are two levels here, then, which characterize the intellectual development of the child, the process advancing from description, in the widest sense, to defining in terms of class-names.

The reason the Binet tests go wrong in this appraisement is shown first by the fact that there are quite intelligent children who make an effort to answer, and remain speechless, or say "I don't know." Of course they know a fork, and if given the proper clew as to what is desired, would not only answer but answer possibly even better than the others. These children size up the problem, however, in too difficult a way, think it a bigger problem than it really is, and therefore stand fazed, just as the aforesaid adult questioned suddenly as to 'truth,' 'a condition,' etc. Change the form of the question to 'Of what is . . . ,' or 'Out of what material . . . ,' or 'Out of what parts . . . ,' and the answer comes easily enough. There are other children again who appear to be making conscious effort to give a good answer, and are heard to repeat to themselves 'is a . . . , a . . . ,' then give it up, and say "I don't know." In these latter cases it is plain that the child strives for a real definition, or, at least the beginning of such, or class-name, believing the type of answer necessitated in the form of the question: "*What is a* . . . so and so*?*"—"*A* . . . *is a* . . . yet being too young to define in terms of genera, can not get clear of the tangle, and sticks. If you would say to this child '*Of what is . . . ?*' or '*For what is . . . ?*' etc., it would

not fail. In other words, a child may feel itself constrained to answer in terms implied in the question—and not be equal to this. Again, it may start the series with replies 'in terms of use'—for, after all, a fork for most of us is *something to eat with*, and a chair, *to sit on*,—and then, having begun that form of reply, feels that it must go through the entire series in this same way—a form it would probably not use in the instance of horse, soldier, mama, etc. (certainly not if an older child) if it thought it was at liberty to alter the form. One notices that some—the more intelligent—children actually do so, as if overcoming a difficulty, and with noticeable relief to themselves. *Here chance, then, plays too big a part to give validity to the test.*

Bobertag, therefore, suggests the following: a longer list of words should be used, and such as can be easily defined, (1) by the younger child—in terms of use, (2) through description, and (3) through class-names or genera (Oberbegriffe), *i. e.*, fork, chair, tongs (Zange), cake, doll, cab, horse, soldier, penny, rose. These words have also a special arrangement: for the first form of definition they become progressively more difficult from first to last; for the third form of defining they increase in difficulty in the reverse order, from the last to the first (rose to cab being fairly easy, doll to fork rather difficult). One then tests out the three forms, or at least the first and third, with this series of words, *intimating the type of answer desired in the manner of asking the question*, or through an example; thus, "What is a fork,—a fork is for . . . ?" or give the entire, 'for eating,' and then proceed with the remaining words. When eliciting the third form, one starts in the reverse order, and Bobertag asks, "A rose and a violet, they are two what . . . ?" then penny and dollar, soldier and hunter, horse and dog, cab and omnibus (or stage), doll and ball, cake and roll, tongs and hammer, chair and table, fork and spoon. If one finally still desires the descriptive form of answer, one provokes this with "How does a . . . look?" etc.

Here, then, we get some idea of the difficulty of evaluating 'definitions,'—a far greater difficulty than the French tests

imply—and the danger of an unfair failure, to which even the intelligent child is thus subjected. It is no easy problem, this gauging of intelligence through defining, it is one scarcely as yet specifically determined. Bobertag's criticism also shows especially well how far from the mark the "cut and dried" Binet test (and testor) may go.

Abstract ideas, (*e*), are not in themselves easily comprehended by normal young children. There is a dawning around nine or ten, and only at eleven we expect a fair understanding of such.[1] It is here that the weak-minded easily reveal themselves, even the higher types, their capacity for abstract thinking being decidedly small. In consequence they show an abnormally meager ability to differentiate the essential from the unessential, the probable from the improbable, etc.[2]

It is interesting to find that studies in word-association[3] also disclosed that children between 6 and 12 or 13 associated, for the most part, *concretely* (and with 'individual' as opposed to 'general' associations) in contradistinction to adults, who associate mostly *abstractly*,[4] and that only from about the thirteenth year on did they appear to grasp abstract logical relations (cause and effect, etc.). Ziehen found that this "concretism" was characteristic of the clever or more talented children up to the twelfth or thirteenth year. The less clever or less bright children gave more abstract associations! This was also substantiated by Meumann.[5] Ziehen

[1] Binet gives the words charity, justice and goodness in the twelfth year tests (formerly in the eleventh year). Two out of three must be passed. At ten years one-third succeed; at eleven the majority. Bobertag's statistics were 11 children out of 36 (31 per cent.) in the tenth year; 20 children out of 36 (56 per cent.) in the eleventh year; 24 out of 32 (75 per cent.) in the twelfth. Meumann gives tests for abstract ideas in the eleventh, twelfth, and succeeding years. At eleven he asks for "pity," "justice," "envy," "friendship," "family," "sin."

[2] See G. Störring, 'Vorlesungen über Psychopathologie,' Leipzig, 1900, p. 399.

[3] See Ziehen, 'Die Ideenassoziation des Kindes,' Berlin, 1898.

[4] So, Compayré also comments: "Does not the secret and art of writing for children lie in the fact of knowing how to avoid abstract and general expressions, the collective and condensed words, and, on the other hand, in knowing how to multiply the concrete expressions, and the details?" ('Evolution intellectuelle et morale de l'enfant,' Chapt. II., Pt. 4.)

[5] *L. c.*, Vol. 1, p. 499–500.

concludes that when a child prematurely approaches the adult type, that is, associates abstractly, it for the most part shows intellectual inferiority. The transition is at 13–14-.

Care must be taken in putting the test questions. Ziehen, for instance, gives a little story embodying the idea of envy, or ungratefulness, and then asks, "What would you call this?" *e. g.*, "A girl sees that another girl has a much nicer dress than her own, and begrudges her this (or, is unhappy because of this), as she would like to have it herself. What would you call that, or what would you say that girl was?" "I once helped a man out of a good deal of trouble, and now I myself am in trouble, and ask his aid, but he refuses to see me. What would you call this (or him)?" Again, one may ask for an example of envy, bravery, etc.,—which however, is much more difficult.

The abstract words taken from Binet's sheet (tests at the age of 13), "poverty, misery," etc., are very difficult. In the revised scale Binet placed them among the adult tests.[1] Taken separately the words are often better understood and defined (a kind of 'catch' being sometimes experienced in the similarity of the sounds and meaning) than when the difference between them is asked.[2]

Under this rubric of the abstract, questions as to *relations* are likewise in place. One may ask for the meaning of 'similar, larger, more, less, almost, mine, cause, effect,' etc.

Such tests may also be made with lists of ten words, having the test-person give 'opposites' to these words, or 'similars,' 'causes,' 'effects,' etc. Or again, questions of logical causal relations may be put, like "Why does one heat up in winter?" "Why does one use an umbrella?" "Why do some people wear glasses?" "What might happen if you would get drenched, or would eat unripe apples?" "Why does a river flow?" "What might be the underlying cause of a person's

[1] An adult (according to Binet) equals "over fifteen years."

[2] Bobertag omits these tests (in fact all those above the twelfth year tests) as being too difficult. Meumann uses the words 'avarice—thrift' and 'mistake—lie' in the thirteenth year, and 'King—President' (Binet's pair) at 15 or over. Binet puts 'King—President' among his 'adult' tests and asks: "There are three differences between the president of a republic and a king. What are they?" (*Cf.* test (*c*).)

being angry, sad, happy?" etc. Here, however, we are stretching comprehension well over into combination.[1] The feeble-minded individual does not grasp the relation of cause and effect, or of purpose, expedience, etc., or the relation between things and a value or worth. Hence they remain impractical, unreliable, unforeseeing and helpless. It is to be remembered, however, that this knowledge comes late to normal children.[2]

Under (*f*) we test *ethical and moral ideas*, that is, knowledge of right and wrong, etc.[3] The method of differentiation may also here be used, and we ask for the difference between mistake and lie, borrow and steal, lend and give, etc. (usually known at 13),[4] though the tonal feeling, which is better or worse, is already felt, according to Ziehen, at the age of 7 or 8. Examples may also be given, which are to be 'labeled,' or the child is asked for an example (of a mistake, lie, exaggeration, deserved and undeserved punishment, fraudulence, etc.). One proceeds best, however, by asking: "What should you do if you see someone lose a pocket-book? May

[1] Thus C. Burt used the analogy test as a reconstruction test. (*Br. J. Ps.*, 3: 1909). Finding analogies really belongs entirely under combination, so does the finding of associations according to cause and effect; but finding logical opposites entails no combinational ability.

[2] Binet asks for 'opposites' (of good, outside, quick, tall, big, loud, white, light, happy, false, like, rich, sick, glad, thin, empty, war, many, above, friend) at 15. He allowed the marking of *half-right* beside the correct ones, and the equivalent of 17 correct answers had to be given to pass. In such tests, however, much depends on the words chosen, for the results vary according to their difficulty. For data on this, see Whipple's 'Manual,' Pt. II., p. 79. Meumann (following the results of Ziehen's association studies) asks for 'reproductions' with logical example (classification of ideas as to cause and effect) at the ages of 11, again at 12 and 13. According to Ziehen they scarcely occur before the eleventh year, and if they do in the eleventh or twelfth they are spare and given slowly (mit geringe Geschwindigkeit). Therefore we must look for the general knowledge of such in the thirteenth or fourteenth year.

[3] These ideas are also *abstract* ideas, and really come under (*e*). It is best, however, to test this group separately, as the special problem of ethical conduct may be involved in the case examined.

[4] More recently ('Geist. d. Kind.,' 1915, p. 68) Ziehen places the knowledge of this (difference between mistake—lie) at 10–12. Should the child hesitate in differentiating 'lie—mistake,' Ziehen asks, "Which is done with purpose?" Or he continues, "When a boy takes jam, and it is discovered, and he says it was his sister, did he lie, or did he make a mistake?" "If a person in adding gets the result wrong, is that a mistake or a lie?" Ziehen especially recommends the question, "Why is the lie worse?"

you keep it, even if you are sure no one saw you find it? Why will you give it back? Why may one not steal? Why does one punish one who steals?" etc. Or one gives an example, and asks: "Would you have done so?" In the case of a delinquent one will ask, "Can one depend upon it that you won't do it again? Why wouldn't you? Why not on a good opportunity?" etc.

One may also ask,[1] "Who, in your eyes, is extremely good, and why do you think so?" "Whom do you hold as very bad, and why?" Or, "Who is your model (or ideal) of goodness, etc., and why?"

We must bear in mind that this examination tells us only if the individual tested has a knowledge of the difference between right and wrong—which does not, however, mean that such knowledge occasions right action, or that the individual will not go forth and do wrong, even though he has a thorough conception of matters ethical and moral. There are children who answer all the above questions correctly, and seem even to 'show feeling' in the matter, and yet, put to the actual test, will lie, steal, and act anti-socially. "To understand, to feel and to act ethically (or morally)," says Scholz,[2] "are three very different things." Nor do these tests touch upon the subject of responsibility for acts done.[3]

A cautioning word must be said regarding the test for comprehension of *time* (g). This is really not a matter of intelligence, but depends much upon instruction. Meumann

[1] Meumann, 'Die Untersuchung der sittlichen Entwicklung des Kindes und ihre pedagogische Bedeutung,' *Zeit. f. Pedagog. Psychol. u. exper. Pedag.*, 13. Jahrg., Heft. 4, p. 212. See also the article of Richter, 'Statistische Erhebung über die Ideale von Volkschulkindern,' same journal, p. 254.

[2] 'Anomale Kinder,' *l. c.*, p. 98.

[3] In the law, responsibility means a knowledge of right and wrong at the time and in regard to the particular act done, knowledge of right entailing (supposedly) right action. This conception is pathetically antiquated, dating back to Socrates. Psychopathology has taught us that this is by no means the case, and, in fact, that such an inference is decidedly erroneous. That mental defect has anything in itself to do with crime is likewise erroneous. These facts will be fully discussed in a paper by the author on 'Delinquency,' to appear presently. On *Responsibility* see the interesting study by Robert Jones in *The Practitioner*, London, Apr., 1913, Vol. XC., No. 4, p. 653. See also 'Die Prüfung der sittlichen Reife jugendlicher Angeklagter und die Reformvorschläge zum Nr. 56 des Deutsch. Strafgesetz,' by M. Levy-Suhl, *Zeit. f. Psychother. u. Med. Psychol.*, IV. Bd., 3 H., p. 146; also *ibid.*, 4 H., p. 232.

placed these questions (under (*g*)) in the 6th year (under his Development-tests, *l. c.*, Vol. II., p. 776). Binet has the child distinguish between morning and afternoon, and asks, "Is it morning or afternoon now?" Not until six, he concludes, is a child absolutely sure. Bobertag, nevertheless, found only 25 out of 55 (45 per cent.) six-year-old children could answer; 87 out of 126 seven-year-olds (or 69 per cent.).

This seems rather startling, for the question appears easy. There is no doubt, however, that the *time sense* develops late in children, and is not very clear for a long time. Ziehen[1] found that between the ages of six and eight children only know such time relations as are drilled into them through their daily routine, and especially that of the school; hence at eight they know the difference between hours and minutes, and also the half hour. But many went astray on the number of hours in a day (even at eight such answers were given as 19, 21, 60 and 23 hours). Ziehen states that for many children the day is not a measure of time, but only the opposite to night. To the question as to how many days a year contained, the answers varied from 20 to 160.

Though Meumann places these tests as stated above (in the 6th year), he nevertheless states elsewhere (Vol. I., p. 304) that he has convinced himself, through repeated questioning of five- and six-year-olds, that they do not understand complicated time conditions. If one says a thing happened yesterday or the day before yesterday or weeks or years ago, this is all merely a vague conception of something past for these children, and it is identically the same with time spans in the future. So if you promise a five-year-old a present fourteen days hence, or in a quarter of or half a year, it is all the same to the child—just something future. The six-year-old may tell you what happens in winter, in summer, etc., but does not grasp the span of time, or the idea of season, as such—or indeed of a quarter or a half year.

Do we ever consider this when we tell young children stories of 'once upon a time' or try to teach them history, or the biblical past? Therefore, we shall not lay much

[1] 'Die Ideenassoziation des Kindes,' Berlin, *l. c.*, p. 9.

stress upon our questions as to time, or take the answers over-seriously.

In testing *Form* (*h*) the De Sanctis set of cubes, balls and pyramids may be advantageously employed, as in tests No. 3 and No. 4 of his series,[1] *i. e.*, one mixes 5 cubes, 3 balls and 2 pyramids, chooses one of the lot, and asks the child to pick out a similar one, then repeats this, choosing another. One then shows a large cardboard on which there are pasted black silhouettes of balls, squares, cubes, triangles, pyramids, cones, crescents, oblongs, etc. The child is asked to point out, with a pencil all the figures which resemble the forms already shown him, and may also be asked for the names of the others on the cardboard. In this test *forms* are recognized, differentiated and recalled, and flat and stereometric figures distinguished. It is accomplished at six.

As a supplementary test to the above, one may employ the 12 cubes of different sizes used by De Sanctis in his fifth test, and enquire as to relations: *i. e.*, which is the largest, the smallest, neither one nor the other, the larger of two, etc. These cubes spread out on the table may be used in testing *space or distance*, (*i*), test I. One asks, "which is nearest, the most distant, point to one neither nearest nor most distant," etc. As test No. 5 in his series, De Sanctis asks, "How many are there, which is the largest, which the most distant?" the time, errors and omissions being noted.[2] One may also,

[1] Sante De Sanctis, 'Types et degrés d'insuffisance mentale.' *Année Psychol.*, XII., 1906. These tests are also discussed by Meumann, Vol. II., p. 325 et seq. See also, 'Mental Development and the Measure of the Level of Intelligence.' *J. Ed. Ps.*, 2, 1911, p. 498, etc. The apparatus may be obtained from C. H. Stoelting Co., Chicago.

[2] In test No. 6 of the De Sanctis series these same cubes are used; they are covered with something, and the child is asked: "Are the largest also the heaviest; are the farthest off the smallest?" This is a test of judgment, and one may give the test and mark it under judgment, among our combination tests in the next rubric. The ability of abstraction and generalization, according to De Sanctis, are also tested in this. The child who can answer these 5 tests, but not the 6th, has, according to De Sanctis, the highest form (the mildest) of feeble-mindedness. But for the adult feeble-minded, the 6th test has been found too easy—they may pass it. So the question has been modified thus: (*a*) "Do large things (les choses grandes) weigh more or less than little ones? (*b*) How does it happen that a small thing sometimes weighs more than a larger? (*c*) Do the distant things appear larger or smaller than the near ones? (*d*) Do they only look smaller, or are they smaller?" *A child passing this test cannot*

however, ask the child the same questions as to the furniture or things in the room. The test is accomplished at six to seven. One may then, test II., draw a line, and have the child divide it so that one part is larger than the other, then, equally in half. This test a child of six to seven can also manage. But when we have test III., a line to be divided in more complicated fashion, in proportions of 1 : 2 or 2 : 4, one finds that only from the twelfth year on is this problem correctly understood, and from the thirteenth carried out just tolerably well.[1] (The comprehension of space and distance in a picture, that is, *perspective*, is a more difficult matter, see further on.)

To test *direction* (*j*), we ask the child to point to the right, to the left (or right hand, left ear), in front, to the back, above, below, etc. Or one may ask (Ziehen): "What is downstairs? Who lives above you?" etc.[2]

be feeble-minded, according to De Sanctis (though he might be one backward in training, or an abnormal character).

De Sanctis' series is composed of six graded tests. Nos. 3, 4, 5, 6 are given above. No. 1 and No. 2 are as follows: No. 1. Six balls of different colors are shown and the child is asked "hand me the red (blue, etc.) ball. (The time taken is noted.) No. 2. The balls having at once been covered and mixed, are again shown and the child asked "Which ball (or balls) did you hand me?" According to De Sanctis, the child who gets no further than the second, has the lowest grade of feeble-mindedness; no further than the fourth, the second grade; no further than the fifth, the highest (mildest) grade.

[1] According to the experiments of Meumann and Giering the visual perception of distances (Raumstrecken) is developed early, and in children of six or seven years of age is almost as keen as in adults. Meumann found that from the 7th year on the test of halving a distance of 2 cm., was accomplished (in the cases he tested) with no error greater than 1/10th mm., in the most cases the error, in fact, being no greater than 1/20th mm. (Meumann, *l. c.*, Vol. I., p. 278.) The question as to which is the longer of two parts (the testor dividing them) is much easier. This may be answered even by defectives. So Ziehen notes ('Geist. des Kindes,' *l. c.*, p. 67) that it is strange how in cases of mental defect the relations-conceptions (Beziehungsvorstellungen) may often not be markedly affected, while the comprehension of colors and numbers is decidedly involved. "I know numerous strongly imbecilic children," he writes, "in whom conception of colors and numbers is almost entirely wanting, and who are able to choose from three paper slips of different lengths, the longest and shortest, or to arrange them correctly as to size." Ziehen also asks ('from memory'): "What is larger, a horse or a sheep," etc.

[2] A child may raise its right hand when it hears 'right,' and left hand when it hears 'left.' This may be automatic memory, each hand having received a name as it were. Through this memory, however, comprehension of direction may be aided. Binet thought one half the children passed his test at five, but that none failed

Tests for the comprehension of *weights*, (*k*), may be carried out in Binet's way. Small cubes of equal size but weighing 3, 6, 12, and 15 grammes each, are given to the child, who is asked which is the heavier (handing him first the 3 and 12 gramme cubes, then the 6 and 15 gramme). At the age of 9 the child is given the entire series, being of 3, 6, 9, 12, 15, and 18 grammes, and is then told that the cubes do not weigh alike, and that he is to place them in order, putting the heaviest first, then the next heaviest, and so on, down to the lightest. Three trials are made, of which two must be correct, nor should the operation take more than about three minutes (Binet).

The final test under *Comprehension* is that of (*l*) *pictures*. This most important test, however, entails comprehension only in the very young, and where the contents of the picture are simply enumerated as single facts, or described. According to Binet, the child at three simply recognizes and identifies the persons and things on a picture, and at the second stage, at seven, describes, giving the characteristics of the persons and things, these latter now being seen in some relation or association, and the description being accomplished with phrases instead of single words. But the meaning of the picture is not yet given. The next level (put at 15 in the revised scale), namely that of interpretation, necessitates combinative ability and therefore belongs in the next rubric (see Combination). It is this interpretative stage in the understanding of a picture that is the most interesting and important, and I have thought it best to reserve its elucidation as well as the general consideration and criticism of the problem of sizing up and grasping the meaning of pictures for our next report. This report will deal with Combination, Attention, Feeling, and Reliability of Memory.

at six. *Therefore it was a decisive test for him.* Bobertag, however, found that but 53 per cent. (only 29 out of 55) passed it, and, in fact, but 93 out of 126 seven-year-olds (74 per cent.). Hence Bobertag places this test in the seventh year. 'In front' and 'behind,' according to the latter, are grasped earlier, 'right' and 'left' being abstracter, while 'above' and 'below' are understood earliest of all, these (last) directions never changing for the child, while the others, if he faces about, are reversed,—hence more difficult to learn.

THE MEASUREMENT OF THE EFFICIENCY OF MENTAL TESTS[1]

BY BEARDSLEY RUML

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The value of a mental test in selecting from a group of individuals those possessing certain academic or vocational abilities is indicated by the amount of relationship which has previously been found to exist between the standing of members of a similar group as given by performances in the test, and their standing in the ability tested for, as determined by the estimation of persons who are qualified to judge, or from the actual achievements of the individuals. In the academic field, when tests are given for the purpose of discovering the mental abilities of students, the standings of the students on the basis of the tests must be related with grades, or with the judgments of ability as given by teachers, before the working value of the tests can be stated. In business and industry, when tests are given to applicants for employment in order that those who are best fitted may be chosen, the efficiency of the tests can be judged only after the results as given by the tests have been compared with the abilities of the candidates, as shown by their subsequent achievements or by the judgments of their superiors.

This relationship, by which the value of a mental test is known, is too complex to be grasped by mere inspection of the data, and symbols of various kinds have been devised to aid in the interpretation of the facts. The most satisfactory measure of the relationship is the coefficient of correlation. As a result, the coefficient of correlation has been widely used in psychology as an indication of the worth of a test; yet when it is computed by the more common methods, *i. e.*, the product-moment method, the method of rank differences,

[1] Mental tests for 'intelligence' and for 'general ability' are to be distinguished from tests in the school subjects, such as algebra and reading tests.

and the foot-rule, it gives an erroneous idea of the true value of the test *in a certain kind of practical situation.*

In these situations to which we refer, mental tests have their greatest use as means of preliminary classification of individuals. The practical problem is to separate individuals into roughly homogeneous groups which will wait for their final internal arrangement upon the development of unmeasurable personal qualities. If a test is to be used in such situations, its value must be determined with reference to these situations. To correlate performance in a test with the exact evaluation of each individual's ability (the method of the three commoner formulæ) is to measure the test in terms of a problem which the test will never be called upon to solve—namely, the determination of the precise ability of each individual. If in the classroom, for example, all that is desired is the separation of students into fairly distinct classes of *good*, *mediocre*, and *poor*, there is no demand upon the test to rank the students in their actual order of merit. The final order of the students will be determined partly by factors which are clearly non-intellectual, and which no test would be expected to anticipate. In business the same condition obtains. The purpose of the test is fulfilled if it succeeds in merely picking out the applicants who are superior, allowing their industry and moral qualities to fix their final rank.

A still more concrete case may be worth describing. Suppose that we have ranked 500 college freshmen, first according to estimates of their ability by their instructors, and second according to their performances in a series of mental tests. The correlation between these standings, let us say, is +.50. The following year we might want to pick out freshmen at the beginning of the school year for advanced divisions in the freshman subjects. The question arises, "Is it possible to make the selection of the brighter students on the basis of their performances in the tests?" Although we know that the correlation between standing in tests and in judgments is +.50, still this coefficient does not tell us how well the tests will pick out a *group* of the more capable

students. For the coefficient +.50 is determined by relating the exact standings of each individual in the two series, standings in tests and standings in judgments.

Suppose the following standings of 16 individuals in the two series, the colon separating the number of individuals to be included in the 'good' group.

Standing in tests........	5	4	3	2	1 :	9	8	7	6	16	15	14	13	12	11	10
Standing in judgments...	1	2	3	4	5 :	6	7	8	9	10	11	12	13	14	15	16

Here, although the correlation by the method of rank differences is only +.61, the efficiency of the tests in picking out the 'good' group is 100 per cent. In other words, accuracy or inaccuracy of the internal arrangement of the groups is equally acceptable for the practical purpose of getting the best students into the advanced divisions. When tests are to be used in situations of this kind, they must be measured by how sharply they differentiate the 'good' as a group from the 'not-good' as a group. Such a measure would give the real practical value of the tests for the specific situation.

The measurement of a test by the true practical situation may be made by use of a formula published in 1907 by Karl Pearson.[1] Fortunately, the measure is exactly equivalent in meaning to the product-moment coefficient of correlation, and it is designated by the same symbol "r." Brown[2] in 1911 called attention to the formula. The exact description of the formula as it is stated in the title of the article in which it appeared is *A New Method of Determining the Correlation between a Measured Character 'A,' of which only the Percentage of Cases wherein 'B' Exceeds (or Falls Short of) a Given Intensity is Recorded for Each Grade of 'A.'* Thus the correlation between performance in tests and the general qualitative divisions of the group as judged may be determined; for the measured character 'A' becomes the test measurements, and the character 'B' given by alternative categories becomes the general qualitative divisions.[3] In the actual

[1] *Biometrika,* 7, 96–105.

[2] W. Brown, 'Mental Measurement.'

[3] Or vice versa, depending upon the information which is desired. If the alternative categories are based upon judgments, the coefficient will tell the value of the tests

situation, to be sure, there are usually three divisions—*good*, *mediocre*, and *poor;* but this difficulty is instantly overcome by dividing the group twice—once into the *good* and *not-good*, and again into the *poor* and *not-poor*. The formula is then applied for each division. In this way it is possible to tell whether the test works more efficiently in separating the *good*, or the *poor*, from the remainder of the group.

Important as the formula is in obtaining a measure of practical efficiency of a test, it has a still greater value. For it may be used to determine just where the division into classes should be made in order that the test may operate at its highest efficiency. Let us consider a group that has been divided into the *good* and *not-good* according to judgments. Clearly this division may be made at any point, and at each point of division there may be a different coefficient of correlation. If the changing values of the coefficient of correlation are plotted for the changing percentages of division (see charts) an excellent indication of the best point of division will be given. If the test is tried out for a sufficiently large number of individuals, 500 or more, this best point of division will be valid for all individuals of that class, and may be taken for use in all further work. The coefficient of correlation at this point of division is the indication of the true practical value of the test.

The presuppositions upon which the formula is based are linear regression—a presupposition for any correlation *coefficient*—and the Gaussian distribution in the alternative variable, here the judgments Strict normality is not essential for practical work but since the accuracy of the result will be affected by the distribution, the measure of the skewness of the curve should always be given whenever it can be computed.

The formula as stated by Pearson is

in selecting a certain percentage as judged. If the alternative categories are based upon tests, the coefficient will tell the relation between a certain percentage as tested and the judgments.

$$r = \frac{\frac{p}{\sigma_1}}{\frac{q}{\sigma_2}}; \quad \frac{q}{\sigma_2} = \frac{\frac{1}{\sqrt{2\pi}} e^{-\frac{1}{2}(y/\sigma_2)^2}}{\frac{1}{\sqrt{2\pi}} \int_{y/\sigma_2}^{\infty} e^{-\frac{1}{2}v^2 dv}},$$

where p and q are the means of the two variates; and σ_1 and σ_2 are the two standard deviations.

In spite of the apparent complexity of the formula, the actual labor in application is very slight. The following ten simple steps will give the coefficient.

1. Determine the mean of the measured character.

2. Determine the mean of the members of the measured character included in the smaller class of the character given by alternative categories.

3. Determine the standard deviation of the measured character.

4. Subtract 1 from 2.

5. Divide 4 by 3.

6. Divide the number of cases in the smaller class by the total number of cases.

7. Subtract 6 from 1.00.

8. 7 equals $\frac{1}{2}(1 + a)$ of Sheppard's tables[1] of the probability integral; secure the corresponding z.

9. Divide 8 by 6.

10. Divide 5 by 9, which gives the correlation coefficient.

Tables of the probability integral (Sheppard's Tables) are essential.

The following problem illustrates the use that may be made of this formula.

Fifty college freshmen were given a series of mental tests. *Required*, (1) a measure of the efficiency of the combined tests; (2) the relative efficiency of the tests in separating the *good* and *poor* groups; (3) the percentage of individuals that should be included in each group in order that the tests may give the best results.

On the basis of the grades of the students, the group was

[1] *Biometrika*, II., or 'Tables for Statisticians and Biometricians,' edited by Karl Pearson.

divided into the *good* and *not-good*, by putting the highest eight per cent. in the *good* group. The correlation with standings in the combined tests was then computed by the formula described in this paper. Again the group was divided into the *good* and *not-good*, but this time the highest twelve per cent. were put into the *good* group. The correlation was found for this second division. Similar divisions were made at the 16, 20, 24, 28, 32, 36, 40, 44, and 48 per cent. points, and the correlation was computed at each division. A curve was then drawn showing the amount of the correlation at each of these points. (See chart.) A curve of the

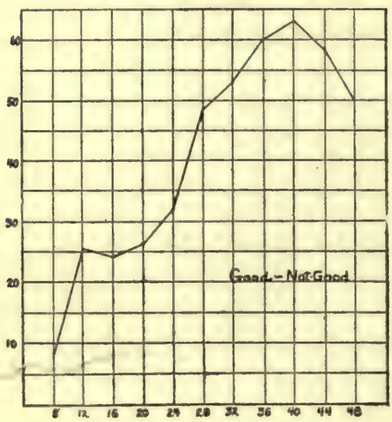

Fig. 1.

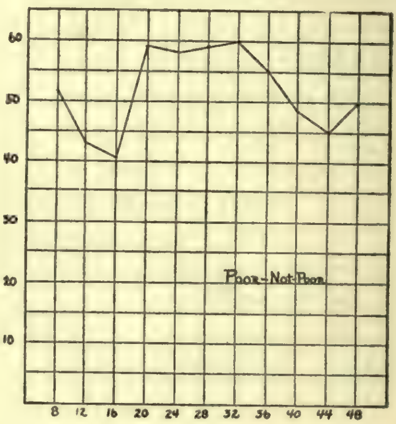

Fig. 2.

Vertical Axis—Coefficient of Correlation. Horizontal Axis—Points of Revision.

various degrees of correlation when the group is divided into the *poor* and *not-poor* was obtained in a similar way.

From these two curves, it may be instantly concluded that if the *good* students are to be separated from the *not-good*, the separation had best be made at the forty per cent. mark, for here the correlation is the highest, +.63. In case the *poor* are to be selected, the division may be made anywhere from twenty to thirty-two per cent., and the correlation will be about +.58.

In contrast with these results, the only information which the product-moment formula gives from these data is that

the correlation is +.43, *a value that is erroneous if the tests are to be used only for separating the individuals into homogeneous groups.*

This problem is one of a type that is common in education, business and industry. Wherever it is necessary to evaluate tests that are to be used in the selection of groups of individuals, the formula described in this paper should be used; for it is then possible to determine what the practical efficiency of the test really is, to weigh the relative accuracies of selecting the *good* or the *poor* individuals, and to determine the best percentage that can be included in either the *good* or *poor* groups.

PENDULUM AND INTERVAL TIMER

BY A. P. WEISS
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In psychological experiments in which a series of distraction stimuli are used, it is often necessary to regulate the duration and sequence of all the stimuli rather carefully. In auditory experiments where stimuli are to be compared successively or simultaneously, it is also important that the duration of the stimuli and the intervals between the stimuli be maintained constant.

There are numerous devices on the market for producing serial or multiple effects, but the apparatus herein described has proved so flexible in its range and so constant in its performance that a brief description seems warranted.[1]

The essential parts of the combination are:

1. An electrically driven duplex pendulum, Fig. 1. The period of this pendulum may be varied without interfering with the driving mechanism or the contacts. The aim in designing this pendulum was: ease of operation, reliability in action, and sturdiness in construction. Graduate students in psychology are often deficient in mechanical ability and flimsy apparatus soon becomes useless.

The pendulum is hung from a knife-edge placed at the middle of the pendulum rod. This knife-edge rests on a grooved steel plate which is fastened to the top of the shelf which carries the driving mechanism of the pendulum. Hanging the pendulum at its center makes it possible to shorten the rod, and by means of the double weights, increase the range of its period of oscillation.

2. A modified Kuhlmann interval timing apparatus, Fig. 4.[2]

[1] This apparatus was constructed by A. P. Freund, mechanician of the physics department, Ohio State University, from sketches and drawings furnished by the writer.

[2] The writer has been unable to locate the original article in which Kuhlmann describes his interval timer.

This is a ratchet device which is tripped magnetically by the pendulum. On an axle coupled to the ratchet wheel axle are mounted a series of sprocket or time wheels whose teeth make contact with a contact spring or brush and thus close an electric circuit. This circuit may then be connected to any kind of apparatus used to produce stimulation of a sense organ (tachistoscope, memory apparatus, adaptation hood, signal light, tuning fork, resonator shutters, telephone receivers or telegraph sounders, gusts of olfactory vapors, time markers, etc.).

The teeth and the spaces between the teeth are so arranged that any desired interval may be used. Six wheels may be used at one time so that six series of contacts may be made. This means that six independent electric circuits are all regulated by the same timing device.

The following are some of the characteristics of the pendulum and interval timer:

1. The pendulum is electrically driven and uses very little current. Its magnet is wound for 110-volt, direct current, and in operation uses about one tenth ampere.

2. The pendulum may be used alone for any of the uses for which a seconds' pendulum is ordinarily used, such as kymograph markers or signal magnets. It needs practically no attention when once adjusted, and if it does happen to stop it does not leave any of the apparatus short-circuited.

3. The current driving the pendulum is insulated from the tripper circuit which drives the timer or other apparatus, so that any short circuits or accidents in the apparatus do not interfere with the driving mechanism of the pendulum.

4. The period may be varied from 1 oscillation in 2 seconds, to 4 oscillations in 1 second. The period is adjusted by shifting the pendulum weights 4 (Fig. 1). For short periods (3 or 4 oscillations per second) the upper weight is removed and then the pendulum acts simplex.

5. Shifting the weights does not interfere with the timer or marker contacts since these are made by the end of the pendulum rod and not by the weights.

6. The length of the stroke of the pendulum can be

adjusted for larger or smaller angles. In general it is best to have the stroke as short as possible to do the work.

7. The pendulum may be arranged to make only a single oscillation with magnetic trip and automatic return. This involves a set of attachments which are not shown in the plates nor described in the article.

8. The current from the pendulum, which trips the interval timer, is independent of the currents which pass through the time wheels and control the stimuli or manipulate the experimental apparatus.

9. The time wheels can be easily changed without disturbing the ratchet mechanism. The phase in which the wheels are set relative to each other is also easily adjusted.

10. One of the time wheels may be used as a series control in the tripper circuit. This will enable the operator to start a series by pressing a key. After the series has been started it will continue automatically and then stop until the operator again starts the next series. In this way a single experimenter may handle an experiment which ordinarily may require two or more operators.

Description of Details

Fig. 1 shows the pendulum complete, set up ready for use. Its height is about 3 feet.

Fig. 2 shows the driving mechanism of the pendulum in detail.

Fig. 3 shows the method by which the pendulum rod makes the contacts of the tripper circuit for the interval timer. These contacts are adjusted on the sector 24 (Fig. 1). They have been found much more satisfactory than the mercury cup ordinarily used for this purpose.

Fig. 4 shows a top view of the modified Kuhlmann timer.[1]

The pendulum rod 1 is made of an iron rod $\frac{3}{8}$ inch in diameter with knife-edge 2 resting in a groove on the steel plate 3. The brass weights 4 can be adjusted up or down on the rod 1 by the set screws 5. It is by adjusting these weights that the period of the pendulum is regulated.

[1] The details of the mechanism are numbered and these numbers are the same both in the description and on the plates.

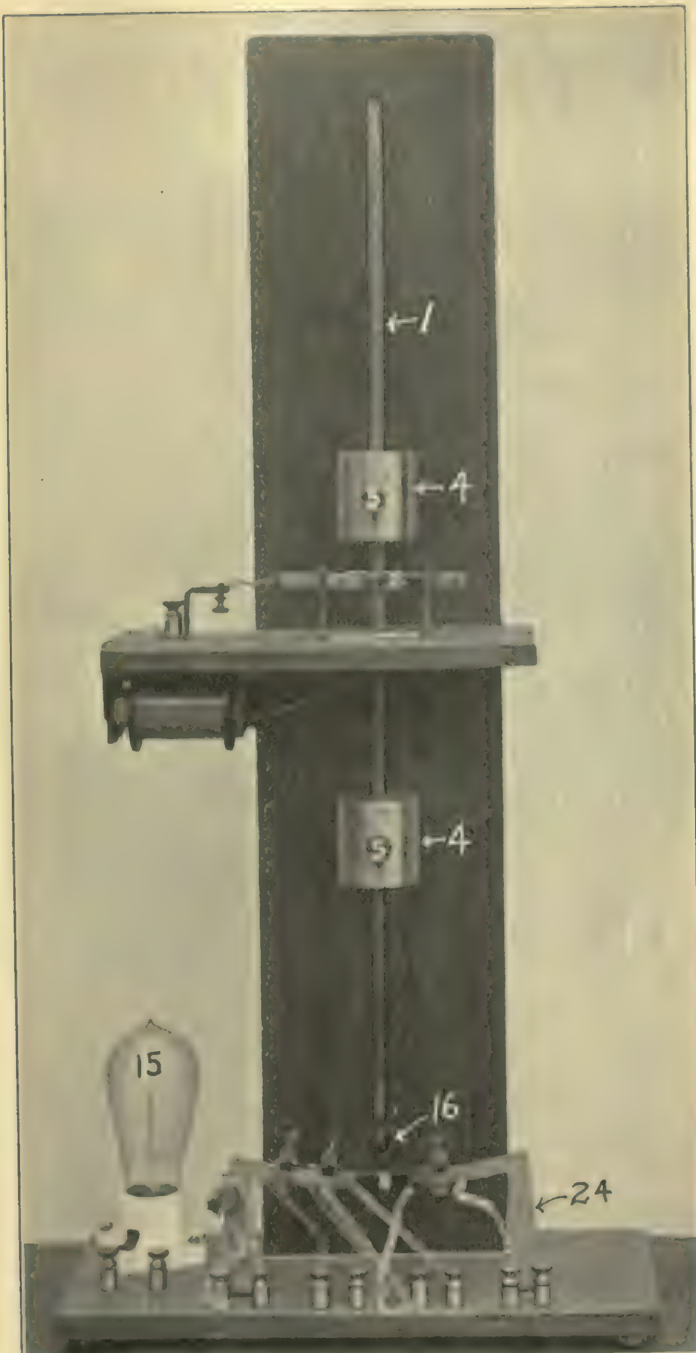

Fig. 1. Front View of Pendulum

The contact bar 6 slides in the guides 7 and makes contact with the plate 8 when the lower weight of the pendulum is to the right. The screws and stops 9 regulate the magnitude of the angle through which a magnetic pull is exerted on the pendulum rod, and this controls the amplitude of oscillation. The magnet 11 is connected with the contact bracket 10, so that when the bar 6 is in contact with plate 8 the magnet will pull on the armature 12. This armature acts on the pendulum rod 1 through the connecting rod 13.

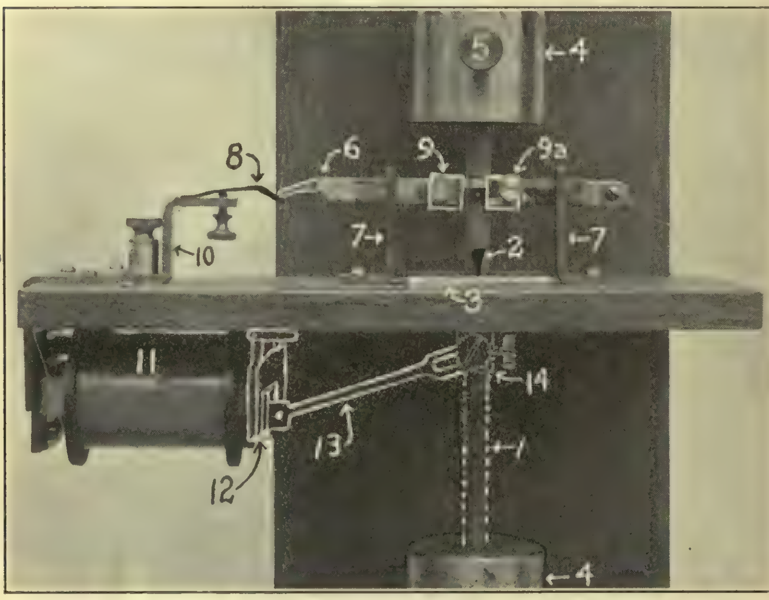

FIG. 2. Driving Mechanism of Pendulum

When the lower end of rod 1 has been drawn toward the left a short distance, an insulated pin in the rod 1 acts on stop 9*a* and draws the contact bar 6 off of the plate 8, thus breaking the electric current which is passing through the magnet 11. The remainder of the stroke is made by the momentum of the weights. This mechanism is practically noiseless in operation. The armature 12 hangs away from the poles of the magnet so that, as soon as the circuit is broken between 6 and 8, it swings freely without touching the magnet cores.

The end of the connecting rod 13 attached to the pendulum rod 1, acts on the clamp 14 which may be adjusted up and down. In this way the leverage of the magnetic pull can be varied. The electric light 15 is a 10-watt lamp connected in series with the magnet 11. At the lower end of the pendulum

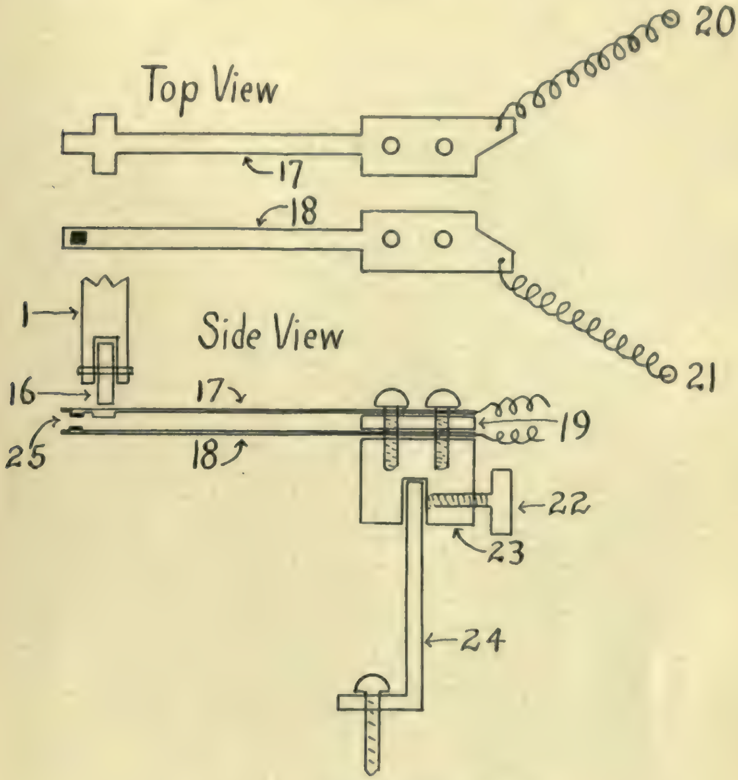

FIG. 3. Details of Tripper Contact

rod 1 is a small hard rubber roller which makes and breaks the circuit through the timer (Fig. 4) by the contacts shown in detail in Fig. 3.

These contacts are insulated from the pendulum mechanism and from each other, and any number may be used without danger of short circuits. Increasing the number of these contacts has the same effect as increasing the period of the pendulum.

The thin bronze springs 17 and 18 make contact at 25 when the rubber wheel 16 passes over plate 17. The contact surfaces 25 are small pieces of silver soldered on to the bronze. The plates 17 and 18 are insulated from each other by the fiber plate 19 and are independently connected

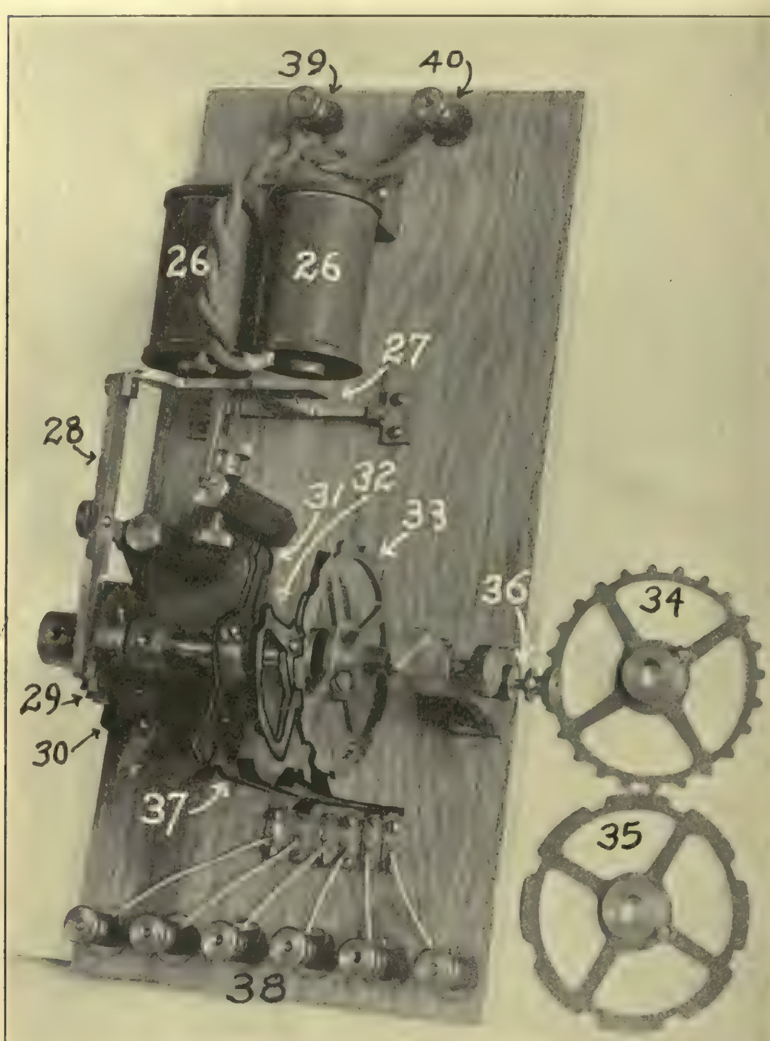

FIG. 4. Top View of Interval Timer

to the binding posts 20 and 21. They are screwed to the brass clamp 23 which can be fastened in any position on the sector 24 by the screw 22. This mechanism makes it possible to close a circuit at any point in the stroke of the pendulum, or where a number of contact clamps are used, to regulate the intervals between contacts. Such adjustments are of greatest value where the pendulum is used as a single stroke pendulum with automatic return.

The circuit which is closed by the pendulum contact 25 is connected to the binding posts 39 and 40 (Fig. 4) and then passes through the timer magnet 26 which acts on the armature 27. The ratchet bar 28 attached to the armature, engages the teeth of the ratchet wheel 29 and moves it one tooth every time the magnet is tripped by the pendulum. The back stop 30 holds the ratchet wheel stationary while the ratchet bar 28 makes the return stroke. The time wheels 31, 32, 33, are mounted on an axle which is coupled to the ratchet axle.

There are 24 teeth in the ratchet wheel and this means that 24 trips of the magnet are necessary to make one complete revolution of the time wheels. The circuits which operate the apparatus used in experimentation, enter the binding post 36 and pass into the time wheels through the axle. The sprockets or teeth of the wheels close the circuits through the bronze spring contacts at 37 and the current then passes to the respective binding posts at 38 and finally to the experimental apparatus. All contacts are designed to work satisfactorily up to 2 amperes on 110-volt circuits. For higher voltages and heavier currents relays should be used.

The time wheels shown in Fig. 4 operate as follows if we assume that the pendulum is set so that it trips the timer once every second:

Time wheel 31 will close a circuit for 1 second and leave it open for 11 seconds; wheel 32 will keep a circuit closed for 3 seconds and open 9 seconds; wheel 33 will keep a circuit closed 1 second and open 1 second; wheel 34 will give an instantaneous current once every second; wheel 35 will

close a circuit for 2 seconds and keep it open for 1 second. As may readily be seen many other combinations of 'open and closed' circuits are possible, especially if the time wheels are operated in tandem.

In an experiment which is now being performed, in which the pendulum and interval timer is being used, it is necessary to present two tones, each one of them twice. One tone is to start as soon as the other stops and the duration of the tones must be 1 second each. To do this satisfactorily by a manual method is practically impossible because it is necessary for the experimenter to record the judgments of the observer and also change the intensities of the tones. By the aid of the combination herein described it is only necessary that the experimenter depress a key long enough to start the series. The tones are then presented accurately as to duration and sequence and the apparatus completes the series automatically and sets itself ready for the next presentation.